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MEDICAL AND SURGICAL

MONOGRAPHS.

BY

MESSRS. ANDRAL, B. BABINGTON, C. J. BECK, BRIGHT, B. C. BRODIE, BURNE, CARMICHAEL, CLUTTERBUCK, CORMACK, DUBOIS, W. FARR, ITARD, LOUIS, MAUNOIR, AND A. S. TAYLOR.

WITH

OCCASIONAL COMMENTS,

BY THE EDITOR OF THE LIBRARY, AND OTHERS.

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TO THE BINDER.

Itard on the Surgical Treatment of Deafness must follow immediately Sir B. C. Brodie on Nervous Diseases.

PREFACE.

Some of the "Monographs"—if they may be so termed—in this volume,—Sir B. C. Brodie's Lectures, Cormack's Treatise on Creosote, Dr. C. J. Beck's Essay on the Application of the Ligature to Arteries, and Dr. Clutterbuck's Essay on Pyrexia, for example,—were originally published as separate works. The rest of the Monographs have been selected from the best transactions of learned societies, and from periodicals that are accessible to but very few of our subscribers.

For certain of them the Editor is indebted to the kindness of professional friends, who have favoured him with translations of valuable memoirs, which, but for their spirited exertions, might have been shut out from the mass of the profession in this country. Those gentlemen will be pleased to accept this public expression of the Editor's sincere acknowledgments.

ROBLEY DUNGLISON,

No. 9 Girard street.

Philadelphia, March 15, 1838.

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ON THE

MORBID CONDITIONS OF THE BLOOD.

BY BENJAMIN BABINGTON, M. D., F. R. S.*

That a fluid which is destined to receive and convey materials for the formation, increase, and repair of every structure in the animal frame—which carries away whatever is useless, and is brought into perpetual contact with the external atmosphere—should itself be subject to morbid alterations, is a notion so natural, so entirely in accordance with what might *à priori* be expected, that, independently of all reasoning and antecedently to all proof, it has existed in the common belief of every age and of every nation.

To preserve a healthy state of the blood has accordingly ever been considered an object of primary importance. The greatest pains have been taken to maintain its purity, as well in the individual as the species—not only in man, but in all those animals which he has domesticated for his use; and there is no belief more generally received than that which attributes the origin of many of the cutaneous eruptions, and of most of the cachectic diseases, to the degeneracy and poverty of this vital stream.

When from this general and popular notion we advance to the more especial assumption, that the origin of all diseases is to be found in the blood and other fluids; when we classify these into hot and cold, moist and dry, or into blood, bile, black bile, and phlegm, and attribute morbid changes and even natural dispositions to the prevalence of one or other of these supposed humours, we quit the belief of the people to follow theories far less tenable, invented at a period when authoritative assertions had the weight of proof, and when the dogmata of a philosopher were preferred to facts plainly recorded in the book of nature.

It would be out of place here to enter into a discussion of the merits of the humoral pathology as compared with the various doctrines which have supplanted it, and to which it is not unlikely that in an improved form it may again succeed.

* From Parts IV. and V. of the Cyclopædia of Anatomy and Physiology.

Under the triple relation of vital phenomena, intimate structure, and chemical composition, as Andral[1] justly remarks, we can draw no definite line of demarcation between the blood and the solids. Physiologically speaking, we cannot conceive that, of these two facts which form a single whole, the one can be modified without affecting the other. Since the blood nourishes the solids, they must necessarily be influenced by its state; and since the solids furnish materials from which the blood is formed, and abstract materials by which it is decomposed, any alteration in the nature or quantity of these must necessarily have its influence on this fluid. Suffice it then to observe, that the further we extend our knowledge of pathology, the less shall we feel inclined to admit the exclusive claims either of fluidism or solidism, and the more shall we strengthen our belief that the animal structure is composed of parts, every one of which may not only partake of disease, but, under certain circumstances, become its cause.

Quitting, therefore, all unprofitable speculations on this subject, we proceed at once to a detail of facts, and to such observations in elucidation of them as occasion may suggest.

Blood may be excessive in quantity—thus constituting a state of plethora in which the circulating system is supplied more abundantly than is needed for the due performance of the functions of nutrition and secretion. A tendency to accumulation in the capillaries and in the different internal organs is induced, and congestion, with its consequences, or actual rupture of the blood-vessels, is the result. Drowsiness, vertigo, headache, epilepsy, apoplexy, mark this state as existing in the head; dyspnœa, and a livid or purple hue of the skin, as affecting the lungs; palpitation and irregular action with syncope mark the ineffectual struggle of the heart to propel its contents. Hemorrhages from the mucous membranes of the nose, the lungs, or the intestines, are often the consequence of congestion in the vessels which ramify on their surface; while indigestion, torpor, and biliary redundancy, are connected with a plethoric condition of the abdominal viscera. Although the existence of such a state, as deducible from the symptoms just enumerated, as well as from the effect which depletion has in removing them, admits of no doubt, it has, nevertheless, not been made the subject of direct proof. The proportion which the circulating blood, even in a healthy animal, bears to its total weight, has not been, and perhaps cannot be, ascertained with precision. Haller collects together many authorities at variance with each other on this point, and at length comes to the conclusion—"Neque dissimulandum est, obiter hæc et vage definiri. Infinita enim procul dubio in ratione sanguinis ad reliquam corporis molem varietas est."[2]

Fat men and animals have less blood than lean, old than young; and yet plethora is oftener found in the former than the latter,

[1] Précis d'Anatomie Pathologique, p. 526.
[2] Elementa Physiologiæ, tom. ii. p. 5.

obviously on account of the mechanical impediment which the encumbered tissue or the rigid fibre offers to the circulation.

The state of anæmia, or a deficiency in the quantity of circulating blood, whether induced by natural or artificial causes, is no less detrimental to health than its excess. Its symptoms are general pallor, weak circulation, languor, syncope with palpitations, oppressed respiration, flatulency, general œdema, and, in extreme cases, effusion into all the serous cavities.

Neither plethora nor anæmia necessarily imply, though they are generally complicated with, some morbid change in the blood itself. We therefore pass them over with this slight notice, referring for further information to the excellent observations of Andral, in his work on Pathological Anatomy.

The circulating blood consists essentially of a homogeneous fluid and red particles; and the former, when removed from the body or from the circulation, separates into a fluid and a solid portion. The solid, when washed and freed from the serum and red particles which are mechanically entangled in its substance, constitutes the proximate animal principle called fibrine. The fluid contains water, albumen, oil, animal extractive, and salts, alkaline, earthy, and metallic.

With the exception of the oil and fatty matter, which, in a healthy state of the blood, do not amount to four parts in a thousand, its constituents are all heavier than water, and something is to be learned by ascertaining its specific gravity. In the information thus gained, however, we are limited to the alternative, either that some one or more of these constituents is in a state of excess or of deficiency, the proportion of water remaining normal, or that the water itself is either superabundant or deficient.

The specific gravity of healthy blood has been variously stated by different authors. Haller makes it on the average 1.052; Blumenbach, 1.054; Berzelius, from 1.0527 to 1.057; Denis, 1.059; but none of these authors note the temperature at which it was taken, although, from their manner of ascertaining it, there must have been considerable variety in this respect. By experiments which I have often repeated with an accurate specific-gravity bottle holding 1000 grains of distilled water, I find that with that fluid four degrees of Fahrenheit's thermometer corresponds with a difference of .001 of specific weight, water being 1.000. Consequently, if one author states the specific gravity of blood at its circulating temperature 98° Fahrenheit, while another states it at 60° Fahrenheit, the usual standard, the former will make it .0095 lighter than the latter.

The heaviest blood of which I find a record among my own observations was that of a man suffering under diabetes mellitus. At a temperature of 87° Fahrenheit it was of specific gravity 1.0615, while that of the serum was under the average standard of health—namely, 1.027 at 60° Fahrenheit, and of the medium proportion to the crassamentum; being, after twelve hours' rest,

as 1000 to 1323. The specific gravity of the crassamentum was 1.088.

The lightest blood which I have met with was of specific gravity 1.031, at 90° Fahrenheit. It was taken from the arm of a female, aged 22, who was bled on account of headache, and had a full pulse of 117.

The red particles being the heaviest of all the constituents of the blood, their relative quantity must greatly affect its specific gravity; and as Messrs. Prevost and Dumas have shown that they bear a general proportion to the degree of animal heat, we might reasonably suppose that, *cæteris paribus*, the heaviest blood would be found in those diseases which are marked by high action and increased temperament. In a fluid so complicated, however, in which every constituent is liable to such variety in quantity, it is difficult to estimate the precise influence of each. I am not aware that any experiments have been made on this subject.

Blood diminishes in specific gravity in proportion to its frequent abstraction, for the red particles and the fibrine are reproduced with more difficulty than the serum or the salts. The serum also becomes lighter from a gradual diminution in its solid contents. A recent paper by Mr. Andrews, in the fifteenth volume of the Medical Gazette, p. 592, proves these facts very satisfactorily by experiments made on calves. They have, however, been long known.

The specific gravity of morbid blood, says Thackrah, differs little from that of healthy blood; but this observation is only true of an average deduced from numerous specimens of blood examined under different forms of disease. It would be equally true, perhaps, according to the same mode of obtaining a result, were we to affirm that the temperature of the body, or the state of the pulse, differed little in health and disease, since there might be as many instances of deficiency as of excess in heat or action. The assertion is not applicable to particular cases, and is, therefore, without value. Blood may be morbid from an undue proportion of any of its constituents, and it will be heavier or lighter than healthy blood, according to the preponderance of the heavier or lighter principles. Where the specific weight is increased, it is generally owing to a deficiency in the proportion of water, as in the blood of cholera and diabetes; sometimes to an increase of fibrine and red particles, as in plethora, gout, and rheumatism.

The following table, containing the specific gravities of blood under several forms of disease, is compiled from a few cases of my own, which were recorded for another purpose. Though short, it will be sufficient to show that considerable variety occurs, and may collaterally suggest, that in determining the propriety of depletion, it may in some cases become important thus to ascertain the proportion of solid matter existing in the circulation. A specific gravity bottle, holding 1000 grains of distilled water, was employed in all the experiments, so that the proportion of serum to clot was not influenced by variation in the shape or material of the receiver.

Sex.	Age.	Disease.	Blood. Sp. Gr.	Blood. Temp.	Serum. Sp. Gr.	Serum. Temp.	Nature of the clot.	Proportion of serum to clot.
Male	64	Dyspnœa and cough . . .	1.048	78 Ft.	1.030	50 Ft.	Gelatinous, buffed, not firm . . .	As 1000 to 1381
Ditto	18	Dyspepsia, with palpitation	1.052	78 „	1.031	55 „	. . . . .	„ 1000 „ 1366
Female	22	Pain in left side from a fall	1.043	86 „	1.026	55 „	Weak, loose, not buffed	„ 1000 „ 1040
Ditto	34	Hæmoptysis	1.045	55 „	1.027	55 „	Cupped, and thinly buffed . . .	„ 1000 „ 745
Ditto	22	Vertigo	1.031	90 „	1.025	55 „	. . . . .	„ 1000 „ 380
Ditto	40	. . . . .	1.051	87 „	1.032	55 „	Not buffed	„ 1000 „ 2906
Male	60	Purpura	1.049	87 „	1.027	65 „	Gelatinous buff, not cupped; serum deep yellow	„ 1000 „ 1495
Ditto	22	Vertigo	1.049	87 „	1.027	70 „	Dark, grumous, and scarcely coherent	„ 1000 „ 1156
Pregnant fem.	24	Vertigo	1.049	60 „	1.028	60 „	Florid red, not firm, nor buffed . .	„ 1000 „ 945
Male	40	Phthisis	1.044	87 „	1.028	60 „	Firm, with bluish white buff . .	„ 1000 „ 960
Ditto	38	Diabetes Mellitus	1.061	87 „	1.027	60 „	Buff doubtful, being drawn in a phial	„ 1000 „ 1323
Ditto	35	Diabetes Mellitus	1.048	90 „	1.024	60 „	Loose, almost diffluent, tough white buff, with milky serum . . .	„ 1000 „ 1292
Same person	..	. . subsequent bleeding	1 049	60 „	1.025	60 „	Ditto, serum equally milky . . .	„ 1000 „ 1479
Female	24	Headache	1.050	60 „	1.029	60 „	Firm, not buffed.	
Male	54	Diseased kidney with coagulable urine	1.041	88 „	1.021	68 „	Not buffed.	

The specific gravity of morbid serum has been much oftener ascertained than that of morbid blood, and it leads to more precise information. The normal proportion of salts does not raise the specific gravity of serum above that of distilled water more than five parts in 1000.[1] The excess beyond this increase is owing to the presence of albumen. The quantity of other animal matter is oo small to be worth taking into the account. Hence the specific gravity of serum indicates, with tolerable accuracy, the quantity of albumen it contains.

In some states of disease, where albumen is rapidly carried out of the system, as in diseased kidneys, in dropsies, and in profuse hemorrhages, the specific gravity of serum has been observed as low as 1.013,[2] whilst in other states, where water and even salts are removed, as in cholera, it is found as high as 1.041.[3]

Neither the specific gravity of fibrine, nor of red particles, has been hitherto stated by authors. The former, by immersion in solution of salt, I find to be 1.079 at 60° Fahrenheit. Some of the latter will fall to the bottom of a solution of specific gravity 1.129, and when agitated with a solution of even specific gravity 1.207, which is the point of saturation, will not rise to the top; but the experiment is not conclusive, for the red particles certainly undergo some change by the addition of salt in solution.

The temperature of the blood is materially influenced by disease. In fevers it is generally, though not always, above the healthy standard. In the cold stage of an intermittent, the temperature of the skin has, according to Dr. Wilson Philip, been observed as low as 74° Fahrenheit, while in its hot stage it has increased to 105°. A corresponding diminution or increase in the temperature of the blood in all probability occurred in these cases. Haller cites authorities to prove that in pleurisy and yellow fever the temperature of the blood has been known to rise to 102° and 104°, in intermittent fever to 106° and 108°, and in continued fever to 109°. Morgagni devotes several pages to the history of a woman, as related in the journal of a cotemporary, Media Via, whose blood flowed in an icy cold state from the arm. The serum of this blood was in small proportion, and of a yellow colour; the crassamentum black and viscid. This person seems to have undergone repeated venesection. Thackrah witnessed a similar phenomenon.

Whatever theory may be adopted respecting the generation of animal heat, it is a fact which is generally admitted, that it is effected through the medium of the blood, that it is, *cæteris paribus*, increased in proportion to the velocity, freedom, and force of the circulation, and that it is mainly dependent for its development upon the presence of the red particles. Wherever these are deficient, either from natural disease or artificial depletion, animal heat is deficient likewise. Chlorotic females, and those who are

[1] Med. Chir. Trans., vol. xvi. part i. p. 57.
[2] Bright's Reports, vol. i. p. 85.
[3] O'Shaughnessy's Report on Cholera, p. 29.

subject to habitual losses of blood, usually suffer from coldness of the extremities. The phenomenon of fainting is always accompanied by diminished temperature; and whenever we cut off the supply of blood from a limb, it loses its natural warmth as an immediate consequence. Plethoric subjects, on the contrary, provided their circulation be unimpeded at its capillary extremities, or in the process of the pulmonary ventilation, are liable to preternatural heat of the surface and profuse perspiration. As an actual diminution or increase in the quantity of the red particles produces a corresponding increase or diminution of animal heat, notwithstanding the natural change of venous to arterial blood, so likewise any cause which impedes that change, although the red particles be not deficient in quantity, will produce a like effect. Thus, in diseases of the heart, in pulmonary obstructions, especially of a spasmodic character, in the cold fit of ague, and in Asiatic cholera, there is a diminution of the natural warmth, although there is no reason to suppose that the red particles are actually less abundant than in health.

Fibrine may undergo alterations in quality during disease. In the healthy state, it is composed of definite quantities of oxygen, hydrogen, azote, and carbon; and it is quite possible that some variety in the proportion of these constituents may give rise in disease to morbid states of that principle. Huxham observes, that in malignant petechial fevers the crasis is so broken as to deposit a sooty powder at the bottom of the vessel, the upper part being either a livid gore, or a dark green and exceedingly soft jelly. De Haen saw the blood in a dissolved state; and in the plague the blood is said not to coagulate.

In some persons there exists a state of constitution, bordering, no doubt, upon passive hemorrhagic disease, in which the blood is observed either to coagulate very imperfectly or not at all. Alarming hemorrhages, from the slightest wounds, are the consequence of such a diathesis, and the most powerful styptics will not always succeed in preventing their fatal termination. Dr. Wardrop, in a small work just published, has collected together several interesting cases of this kind, and from some of these it is demonstrated that such a condition may exist in many members of the same family, and even sometimes become hereditary.

In the dead body, blood is sometimes found in a liquid state, resembling water, holding in suspension a red, brown, or black colouring matter. In this case, according to M. Andral, it has been demonstrated chemically that it still contains fibrine, but altered in its character, so as to be no longer coagulable. This dissolved state of the blood, observable after death, is probably the same as that which exists in sea-scurvy, in putrid and typhus fevers, and in the latter stages of fatally terminating diseases, characterised by defective nervous energy. It is matter of more common observation, however, that fibrine alters materially in its relative quantity. We often find that the clot is large in proportion to the serum, which may indeed arise from its being loose and

defective in contractility, so as to contain a large portion of fluid, or from its holding entangled among its meshes an unusual number of red particles; but it will often also arise from there being a more than ordinary quantity of fibrine present, in which case it will be firm and contractile as well as voluminous. Blood thus circumstanced is said to be rich and thick, and is generally met with in those whose complaints are connected with a plethoric habit.

A deficiency in the proportion of fibrine is likewise not unfrequent among those who suffer from complaints of debility, or who have lost much blood by natural or artificial depletions. In this case the clot is small, and has but little contractile power.

It is, I conceive, a possible case, that the fibrine may separate imperfectly or not at all, in consequence of an augmented proportion of salts, which, out of the body, we know to be capable of suspending coagulation altogether. The continued use of alkaline remedies will probably tend to produce a like effect.

Fibrine coagulates the more speedily in proportion as the circulating and nervous systems become more feeble. The experiment has been repeatedly made with animals that are killed by bleeding, and the last portions of blood invariably coagulate soonest. "The principle of the blood's speedy concretion in debility is important in a curative point of view. The first natural check to hemorrhage is known to be the formation of a clot on the mouth of the vessel. If the longer the hemorrhage the less had been the disposition to form such a clot, the wounded on the field of battle, and those injured by common accidents, who cannot promptly procure the aid of a surgeon, must inevitably have perished."[1]

One of the most remarkable and frequent deviations from the normal condition of blood removed from the body by venesection, is the occurrence of the buffy coat, which is a layer of fibrine occupying the surface of the crassamentum. The blood, whilst circulating within its vessels, consists, as I have already remarked, of a fluid which I have elsewhere ventured to call *liquor sanguinis*, and of insoluble red particles. These being in constant motion, are uniformly diffused throughout this *liquor;* but their specific gravity being much greater than that of the medium in which they are suspended, they have a tendency to gravitate whenever that motion ceases. In healthy blood, the fibrine coagulates so quickly, that the red particles have not time to subside, so as to leave any portion of the *liquor* entirely free from them. By protracted fluidity this result is effected; the red particles do then gravitate to a greater or less depth before the *liquor* separates into two parts. A general coagulation of the fibrine at length occurs, and a clot is formed. That part of it through which the red particles had fallen, becomes a layer of fibrine, free from colour, and merely having some serum mechanically retained in its meshes, while the subjacent portion is of intense depth of shade, especially at the

[1] Thackrah, p. 188.

bottom, and of less than ordinary cohesion. In extreme cases, such an abundance of red particles reaches the bottom of the vessel, that they are there found in a state of fluidity. The buffed layer sometimes assumes a cupped form, which is clearly owing to unequal contraction. The upper surface being freer from intervening red particles, contracts more powerfully than the under, and a concavity of the surface is the necessary consequence. Where, however, the contraction is weaker, the weight of the subjacent red clot, which is one and the same mass with the upper colourless portion, weighs this down, and keeps it in a horizontal position.

The crassamentum of arterial, as well as of venous, blood has frequently been observed to exhibit a buffy coat. It is rarely seen in blood extracted by cupping-glasses, and never in that pressed from leeches. It occurs in the lower animals, and is observed as frequently in the horse as in the human subject; indeed, from the quantity of blood usually drawn from that animal, it is still more strikingly apparent, being occasionally several inches thick. It has been denied that the cupped appearance is ever met with in the blood of the horse; but if this be received into a sufficiently small vessel, it will be in some instances as complete as in blood taken from the human subject. There are varieties in the appearance of the buffed coat which it is worth while to notice. It is generally of a firm uniform consistence, and of a light yellow or buff colour, whence its name. Sometimes, however, it is of a more spongy texture, and of a white or bluish, and more transparent hue. Two layers of buff are occasionally seen; the upper soft and friable, the inferior more compact. "There is a difference," says Sir Gilbert Blane, "in the appearance of the blood when sizy, perhaps not sufficiently insisted on by practical writers; for though there should even be a very thick buff, yet if the surface is flat, and the crassamentum tender, no great inflammation is indicated in comparison of that state of the blood wherein the surface is cupped, the crassamentum contracted so as to form the appearance of a large proportion of serum, and where it feels firm and tenacious, though perhaps but thinly covered with buff."[1]

From the examination of several specimens of buffed blood, I was at one time led to believe that its serum was always deficient in its due proportion of albumen; but this I have since found not to be the case, having met with blood thickly buffed, the serum of which, at 60° Fahr., had a specific gravity of only 1.024, and with another specimen where the layer of fibrine was equally thick, of which, at the same temperature, the serum had a specific gravity of 1.040. Dr. John Davy examined the specific gravity of buffed blood in eleven cases. In five of them, in which the buffy coat was slight, the specific gravities were 1.047, 1.051, 1.054, 1.055, 1.054; in five others, in which the buffy coat was moderately thick, the specific gravities were 1.044, 1.038, 1.052, 1.056; and in one

[1] Blane on the Diseases of Seamen, note to page 314.

instance, in which it was thick, the specific gravity was 1.057. Taking the mean gravity of healthy blood at 1.044, which I believe will be found correct, it would thus appear that the buffy coat is more frequent in blood above than below the mean weight; but it is also clear that it may exist in either state, and the number of experiments is not sufficient to lead to any conclusive result.

De Haen, Hewson, and others, have met with cavities in the crassamentum of buffed blood, containing clear fluid (liquor sanguinis), which, on being evacuated several hours afterwards, separated into fibrine and serum. This fact is analogous to that of fluid blood having been found by Hewson in the heart of a dog thirteen hours after death, which blood, on being removed, coagulated soon after exposure to the air. A similar coagulation will occasionally take place in fluid blood taken from the human heart several hours after the extinction of life.

The remote cause on which the occurrence of the buffy coat depends, appears to be an increased action in the circulating system, dependent on increased nervous energy, and this is capable of being very speedily excited. Thus it has happened[1] that blood from the same orifice drawn into four cups has exhibited this appearance in the second or third cup, and not in the first or last, the difference being plainly owing to a faintness felt at the commencement and termination of the venesection. Thus, also, the blood of healthy horses, drawn immediately after a smart gallop, while the circulation is powerful and rapid, will exhibit a buffy coat, while that previously abstracted will, of course, show no such appearance. Scudamore, it is true, arrived at an opposite result in the case of a young man whom he bled, and after causing him to run two miles, bled again. Neither before nor after the race was the blood buffed; but it is obvious that such severe exercise, after depletion, would exhaust rather than augment the powers of the nervous and circulating systems. Accordingly, he found the proportion of fibrine diminished in the blood last drawn, while the specific gravity of the serum was increased from 1.030 to 1.035, thus showing how large a quantity of moisture must have been carried off by perspiration.

The buffy coat, as might be anticipated from its cause, is usually found in connection with those diseases, and even conditions of health, in which vascular action is preternaturally increased—in the active stages of peripneumony, in pleurisy, in inflammatory fever, scarlatina, and the eruptive diseases generally, and very uniformly in acute rheumatism. It is also occasionally, but not always, met with in the blood of pregnant women, in persons of sanguine temperament and full habit, and those who resort to frequent blood-letting; in chronic rheumatism, gout, enlargement of the heart, and other affections where no inflammation exists. On the other hand, it may be absent even in the most intense inflammation; for the circulation may be so overcharged, either actually

[1] See Hewson on the Blood, vol. i. p. 82 et seq.

or relatively, or the nervous power so oppressed, that the requisite degree of propulsive force is not exerted by the heart and arteries, nor the vital energy on which slow coagulation depends, imparted to the blood. In such instances, the buffed coat generally appears on a second or third repetition of venesection.

Louis found the blood covered by a firm thick buff at each bleeding, in nineteen cases of fatal peripneumony out of twenty-four. In two fifths it was cupped. In fifty-one out of fifty-seven cases of recovery the blood was buffed, and in twenty-three cupped. In nine tenths of rheumatic patients, the buff was firm and thick.

The form of the receiving vessel, the degree of motion to which it is subjected, and the size of the orifice in the vein, materially influence the phenomenon. M. Belhomme, the experimenter under M. Racamier, has made about one hundred and fifty experiments on blood, drawn in health and disease. He has come to the conclusion, that a medium orifice, one line in the vein, a strong, rapid, and continuous jet, in the form of an arch, and a narrow vessel for the reception of the blood, are the external circumstances most favourable for producing the buffy coat.[1]

Fibrine is more abundant in buffed than in healthy blood. Dr. Davy, from his observations, infers that there is no constant relation between the appearance of this covering and the proportion of fibrine in the crassamentum, yet his own tabular report contradicts him. "From all the examinations we have made," says Thackrah, who has made many experiments to determine this point, "I infer, without hesitation, that buffed blood contains a considerably greater proportion of fibrine than healthy blood." This is a fact of much interest and importance, for as very slight and sudden causes may give rise to the formation of a buffed coat, we are thence led to infer, that the quantity of insoluble matter, which separates from the liquor sanguinis by coagulation, is variable, and that there is, so far, reason to believe that fibrine and albumen are principles convertible into each other.

In connection with the appearances dependent upon the slow coagulation of fibrine, I may here notice the occurrence of what have been termed polypi, or more recently and correctly, false polypi in the heart and larger vessels. These are so common, that, as Haller observes, scarcely a body is met with in which they do not exist. They are found in both auricles and both ventricles, and in the larger arteries and veins, as well of the trunk as of the extremities. They consist essentially of fibrine, and partake of all the varieties that are observable in the fibrinous coat of buffed blood. Haller affirms, as usual, supporting his opinion by numerous authorities, that these have been known to exist even during life, not only in man, but in the larger warm-blooded animals, and adverts to a disease, *la gourme*, common among horses, which arises from a coagulation of the blood in the large arteries and veins, and in the heart. Thackrah is of the same opinion; and

[1] See, also, Med. Chir. Trans., vol. xvi. p. 296, note.

Dr. George Burrows, who has made the changes which take place in the blood, when its circulation is stopped in the living body, the subject of the Croonian Lectures of the present year, states that "there can be but little doubt that, in some cases, the blood coagulates in the heart during life. The firmness of the clots found in its cavities after death—their close adhesion to the lining of the heart—the presence of various fluids in the centre of these clots—the occasional organisation of the coagulated masses, and their partial conversion into structures which are similar to new growths in other parts of the body—are facts which lead us to the conviction that the blood often coagulates in the heart long prior to death."

That such coagulation may take place during life, I am willing to admit, but I am by no means led to the conviction that such an event *often* occurs. To the formation of a firm coagulum, I am persuaded that rest is absolutely necessary, and I must consider it as a very rare occurrence that the contents of the cavities of the heart should be at rest during life. The usual appearance of false polypi is such as would take place in blood that coagulated very slowly, whether in or out of the body. Mr. Thackrah has proved that the blood when at rest coagulates much more slowly in living vessels, among which, his experiments include vessels recently removed from living animals,[1] than in those that are dead; and I conceive that the human body, long after the heart has ceased to beat, and when it is, in the common acceptation of the term, dead, is still endowed, like the vessel just separated from the living animal, with a sufficient share of vitality to keep the blood, which is in the heart and larger vessels, in a fluid state, and thus to permit its coagulation to take place, at length, far more slowly than under ordinary circumstances. The following fact will, perhaps, be considered to have some interest as bearing on this point. I was engaged in the post-mortem examination of a gentleman who had died apoplectic from softening of the brain, which had given rise to effusion into the ventricles, and under the pia mater; and being desirous of examining the fluid thus effused, I collected it with a clean sponge, by successively dipping this into the ventricles, and squeezing the fluid into a small cup. With a view to increase the quantity, I used the sponge also in soaking up some of the same fluid which had been caught in the calvaria, but was somewhat tinged with red particles. The cup was set apart till the conclusion of the examination, which lasted an hour and a half, when, on proceeding to transfer its contents to a phial, I was not a little surprised to find that a bulky clot, of a rose colour and perfectly distinct, was formed in the fluid. The examination in question took place twenty-two hours after death. As long as galvanism will stimulate the muscular structures to convulsive movement, so long, at least, may we conceive such a portion of vitality to remain as will influence the state of the blood. The fluid thus circumstanced, exhibits the same phenomena, though in a more

[1] Thackrah on the Blood, p. 85, expt. lii. and liii.

marked degree, which we observed in buffed blood out of the body. The red particles subside, and leave the liquor sanguinis free from colour. In due time this separates into fibrine and serum; the coagulation takes place uniformly and universally, and in the larger cavities and vessels a colourless clot is left, which is moulded into their exact shape. The serum drains off, and washes away the red particles into the more depending and distant vessels. Thus it is, that where we find polypi in the heart, we often find the descending aorta and the vena cava inferior filled with fluid, in which there is no fibrine at all. The firmness of a polypus affords no proof that it existed during life, or rather before respiration and circulation had ceased; for what can be firmer than the buffed coat which we often see formed out of the body? Its close adhesion to the lining of the heart is generally in appearance only, and is occasioned by the exactness with which it has adapted itself to every cavity and sinus, and enveloped every column, and the force with which the heart itself has contracted upon it. The presence of fluid in the centre, however difficult to account for, is also occasionally met with in the crassamentum of blood abstracted from the arm;[1] and even purulent matter, said to be found in false polypi, is occasionally formed out of the body. "In some rare cases, I have seen the fibrine," says Andral, "assume a different aspect. The blood had no clot, and, instead of it, we observed at the bottom of the basin a kind of homogeneous purulent matter, of a deep brown or dirty gray colour, and rather resembling sanies than blood."

With regard to the existence of organisation, it seems to me that sufficient distinction has not usually been made between those cases where the lining membrane of the cavities of the heart or vessels has been ruptured, and which in so far are of the character of aneurism, and those where that membrane has remained entire. I am willing to admit, that where there is a lesion of surface, adventitious growths will readily spring from it; but their substance is furnished from the structure beneath, and not from the circulating fluid. As an instance, I may mention the case of a youth, who, being in perfect health, received a sudden shock from the unexpected discarge of a pistol close to his ear. He immediately felt conscious that something had given way in his heart, and from that hour suffered from palpitation, occasional syncope, with the usual symptoms of obstructed circulation, and died of general dropsy at the end of eighteen months. On examination after death, the mitral valve was found to be obstructed by a fringe of excrescences, originating, no doubt, from a rupture of the valve itself, which had taken place at the time of the sudden surprise. This kind of growth, as well as that which is formed on the inflamed surface in endocarditis, has a sufficiently evident origin. We can also readily account for organised structures, arising from aneurisms, of the heart or arteries, accidental wounds of the latter

[1] See Hewson, p. 69 and 70.

vessels, ruptures of their inner membrane by ligatures, or its destruction by inflammation. I can, however, imagine nothing more unlikely than that an insulated mass of fibrine, owing its origin to the mere coagulation of the blood from rest, and therefore only by gravitation brought in contact with the sides of the vessel which may contain it, should assume an organised structure, and that, too, at a time when the powers of life are so much enfeebled that the heart itself ceases to perform its office. I have looked carefully for unequivocal signs of vitality in these false polypi, and I confess that I have never been able to satisfy myself of its existence.

The albumen has not been demonstrated to be subject to alteration in quality. Its distinguishing characteristic of coagulating by heat, is preserved even after it has become, in the highest degree, offensive from putridity.[1] It may be excessive or defective in proportion, and M. Gendrin has shown that under inflammation of the system, the serum contains twice as much albumen as in the healthy state. Andral affirms, that even by the touch, we may, from its viscidity, recognise serum that is surcharged with albumen. Its specific gravity, however, of which the French writers seem to take little note, would be a far better guide, and would indicate alike the defect as the excess of this principle. M. Gendrin has occasionally observed a mucous layer either at the bottom of the serum, or suspended in it. This is, in all probability, a minute portion of fibrine, separating in the form of a flocculent cloud; for serum is capable of holding a certain portion of fibrine in solution, which after a time separates from it. This was first proved by Dr. Dowler,[2] who, on pressing the buffed coat of blood, extracted from it a liquid serum, which, on being allowed to rest for some time, exhibited signs of coagulation. With regard to the relative proportions of the serum and the clot, I have proved elsewhere[3] that this depends much on the vessel into which the blood is received. I shall show experimentally, however, in treating of diseased kidney, that an opposite state to that above alluded to as occurring, according to M. Gendrin, in inflammation, takes place under certain forms of disease where albumen is passing out of the system by the urinary passages. Thackrah lays it down as a law, to which he has found no exception, that in all cases in which the proportion of fibrine is considerably above the normal standard, the solid matter in the serum is below it. He cites ten examples in proof of his assertion, and puts it as a question, whether we may not hence suppose that the albumen is taken from the serum for the formation of fibrine? The fact itself, however, requires confirmation, being in direct opposition to M. Gendrin's statement, that the proportion of albumen is greatly increased in an inflammatory condition of the system, which is precisely that condition when, in general, we

[1] See a paper by M. Vauquelin, in the 16th vol. of the Ann. de Chimie, new series, p. 363.

[2] See Med. Chir. Trans., vol. xii. p. 89.

[3] Med. Chir. Trans., vol. xvi. p. 296.

find buffed blood, and, therefore, according to Thackrah, an increase in the proportion of fibrine.

The hæmatosine is the least destructible of all the elements of the blood, retaining its qualities in that fluid after having been kept for several years. It is liable to much variety in its proportion; and, in all those diseases and states of system in which hemorrhages occur, it gradually diminishes, at least to a certain point, in proportion to their extent and duration. In what part of the system the red particles are elaborated, remains, for the present, a mystery. That they are reproduced slowly, is manifested by the fact, that those who have suffered large losses of blood, remain exsanguine for many months, or even years, afterwards. The same conclusion may also be deduced from the circumstance, that women have a smaller proportion of red particles than men, the difference having been shown by M. Lecanu to be attributable to the monthly loss which they habitually experience. Besides change of colour, to which the red particles are liable during disease, and which, among other causes, may arise from an altered proportion in the saline matters contained in the blood, they also appear to undergo structural alterations. In fevers, in malignant diseases, in sea-scurvy, in cases of poisoning, and of asphyxia from lightning, a permanently liquid state of the blood occurs, wherein the colouring matter of the globules appears to have lost its character of insolubility in the serum, and to be capable of percolating those tissues which are otherwise destined to contain it. Passive hemorrhages, petechiæ, and ecchymoses, are the results during life; and, after death, a stained condition of the lining membrane of the heart, the arteries, and veins, which has often been mistaken for vascular congestion of these parts.

The oil or unctuous soft solid which is now ascertained to be one of the constituents of healthy blood,[1] is liable to morbid increase under various forms of disease. Morgagni cites two cases of malignant fever in which the serum was milky. Hewson, besides enumerating instances to be met with in authors, gives three cases sent him by medical friends: one of amenorrhœa, with dyspepsia and vicarious discharge of blood by vomit and stool; another of violent and continued epistaxis; and a third of dyspepsia, with slight asthma. In all three cases there were symptoms of plethora; but milky serum is by no means necessarily connected with this state. The most marked instance that I have met with, was in a case of diabetes, where bleeding was several times repeated at long intervals, and on each occasion the same morbid condition of serum was observed. This was quite opaque, and nearly as white as milk; and on standing for a few hours, a film of matter, resembling cream, covered the surface. The clot could not be seen when it was scarcely a tenth of an inch beneath the surface. It had a firm, very thick, white coat of fibrine, and the red particles were almost diffluent beneath. The patient, a female, could

[1] Med. Chir. Trans., vol. xvi. p. 46.

not be called plethoric, having been the subject of her emaciating complaint more than a year and a half. Milky serum, though of a far less marked character, having occurred in persons who have been bled shortly after making a hearty meal, the notion has been entertained that it is owing to the passage of liquid chyle into the circulation. This was Haller's opinion, while others have attributed its appearance to admixture of fat. To the former notion it may be objected, that whereas it is certain that the milky appearance of serum is owing to the presence of oily particles, it is very doubtful, from the discordant opinions of eminent chemists, whether the chyle contains more oily matter than the blood itself. Berzelius, indeed, makes its solid part to consist of more than twenty-one per cent. of fat, and Raspail considers it as differing little from milk. Prout, however, whose analysis is adopted by Turner, only admits an unappreciable trace of oily matter in chyle, and makes its composition differ little from blood, except as respects the absence of red particles. In milky serum the oil exists in superabundance, at the expense of the albumen, which, in all the specimens I have examined, has been remarkably deficient in proportion, its specific gravity varying from 1.019 to 1.024. This circumstance naturally leads to a question whether this oil may not owe its origin to some chemical change in the albumen itself, of which it seems to supply the place. The "remarkable blood," described by M. Caventou,[1] and alluded to by M. Raspail,[2] which was evidently nothing more than blood with milky serum, affords additional ground for supposing that such a change takes place. "This blood, issuing from the vein, was turbid, of a pale dirty red colour, and became marled and of a whitish red as it cooled in the basin, and some drops, which fell on the floor, assumed this colour in a few seconds, and looked like drops of chocolate made with milk. After half an hour a coagulum of moderate size was formed in it, which floated in a large quantity of a white opaque fluid exactly like milk." Raspail, who had evidently never seen a marked example of milky blood, gives the following fanciful explanation of the appearance. "Under the influence, or in the absence of one of the causes which together produce the circulation, an acid had been formed, which, saturating the alkaline menstruum of the albumen, had caused it to coagulate. Now this irregular coagulation could not take place without disguising the colour of the blood, and rendering it rose-coloured, while it would give the serum the appearance of milk." If the albumen had really been coagulated by an acid, a distinct clot would not have been formed by it, but a curdled precipitate; nor would the serum have borne any resemblance to milk. But what is important as confirming my view respecting the conversion mentioned above, M. Caventou, to his great astonishment, could not find any albumen in the milky serum here described. The probability of this change is heightened by the consideration that something analogous must necessarily occur in the formation of

[1] Annales de Chimie, vol. xxxix. p. 288. [2] Sect. 941.

true milk, the oil of which, when separated as butter, and melted to clarify it from the curd, remarkably resembles the oil of milky serum.

The attention of pathologists to the salts of the blood, which, considering the visible effects they produce on this fluid, had been strangely neglected, has of late years been roused by the observations of Dr. Stevens, who certainly may claim the merit of having advanced our knowledge of facts on this subject. It appears that in the last stages of tropical fevers, the saline ingredients of the blood are so much diminished that they are no longer capable of giving a red colour to the hæmatosine. The black blood that is found in the heart after death from either the climate fever or the African typhus, remains black even in an atmosphere of pure oxygen, but it instantly changes colour when we add it to a clear fluid that contains even a small portion of any neutral salt. Nor is it in fever alone that this deficiency of salts is observed. Dr. O'Shaughnessy has shown that it likewise exists in malignant cholera, and it is probable that in sea-scurvy, and in those analogous diseases produced by want and unwholesome nourishment, a similar state occurs.

The saline matters may be in excess as well as in defect, and this is marked by excitement of the circulating system, and either local determinations or general febrile disturbance. The stimulant effect of saline springs has been known time out of mind, while the thirst and heat produced by the too copious use of common salt is in every body's experience. If we couple these facts with the certainty that the neutral salts will pass unchanged through the circulation so as to omit of detection in the urine, we may infer that their superabundance in the blood is not only a possible, but, in all probability, a frequent occurrence. They are occasionally found after death deposited in a crystalised form, as was observed by Sir Everard Home, who, in dissecting an aneurismal tumour, found a mass of crystals, which were analysed by Mr. Faraday, and are stated to have been salts usually met with in the blood.

Having thus concluded such remarks as the present state of our knowledge has enabled me to offer respecting the morbid changes which take place in the separate constituents of the blood, I now proceed to notice some of the more important diseases in which those changes have been observed to occur.

Inflammation.—The usual appearances of blood in inflammatory diseases have already been described in treating of the buffed coat. The crassamentum is commonly supposed to be increased in bulk, but this is somewhat doubtful; and indeed it so much depends upon extraneous circumstances, such as the form of the vessel in which the blood is received, the time allowed for the contraction of the clot, which it is well known goes on for many hours, and even the quantity abstracted, that no accurate deduction can be drawn from its appearance. The collection of the fibrine itself is easily effected, and it will thus be perceived, that, under inflammation, it is more abundant than in the normal state.

Scudamore has made numerous experiments on the relative quantity of fibrine contained in healthy and diseased crassamentum, and the following short list selected from them satisfactorily establishes this fact.

In 1000 grs. of clot, as deduced from eight specimens of healthy blood, average of dry fibrine . . . . . Maximum 4.43, minimum 2.37	3.53 grs.
Slight pleurisy, blood slightly buffed	7.05
Pain in the side, ditto	11.37
Cough	7.24
Acute gout, blood not buffed, firm texture	5.88
Disease not named, clot compact, buffed, and cupped	12.41
Ditto	13.73
Average	9.62

Mr. Jennings, in his report on the blood in the Transactions of the Provincial Medical Association for 1834, likewise gives a table, the result of which is, that in eight cases of inflammation the proportion of fibrine in the blood was increased from 2.1, which is Lecanu's standard of health, to 9, 8, 11, 6, 5.3, 7, 6.9, 7; average 7.525, and that the alkaline salts were diminished from 8.37, the healthy standard, to 4.9, 4.8, 5.1, 4.3, 4.2, 4.4, 4, 5.6; average 4.61.

Among all the varieties of inflammation it is in acute rheumatism where we find the blood most decidedly loaded with fibrine. Owing to the powerful action of the heart and arteries, it is intensely arterial in character, and sometimes issues from the vein with a distinct pulsation.

Fever.—In those fevers which arise from marsh miasmata or from contagion, it is an opinion held by Dr. Stevens, and supported at great length in his work on the blood, that a diseased condition of that fluid is the first in the train of symptoms which occur, and the immediate cause of those which follow. The blood itself, says Dr. Stevens, is both black and diseased even before the attack. During the cold stage it is very dark. When first drawn it has a peculiar smell, and coagulates almost invariably without any crust. There are black spots on the surface of the crassamentum, the coagulum is so soft that it can easily be separated by the fingers, and during its formation a large quantity of the black colouring matter falls to the bottom of the cup. In the hot stage it becomes more red, and, in some cases, it is even florid for a time, but during the remission it is darker in colour than the blood of health, and decidedly diseased in all its properties. In milder cases, the blood which is drawn may coagulate without a crust on the surface; but in the more severe forms of this fever, when the blood was drawn at an advanced period of the disease, a part of the albumen coagulated on the surface of the fibrine, and formed a diseased mass, which, in appearance, had a greater resemblance to oatmeal gruel than to blood drawn from a healthy person. The serum which separated was also diseased; it had a brownish colour, and in some cases an oily appearance, which is never met with in the

clear serum of healthy blood. In the climate or seasoning fever of the West Indies, which is not considered contagious, but a fever of excitement, the blood drawn in the first stage flows from the vein with great force, but is neither cupped nor buffed. It is so florid, being charged with salts which ought to have been removed by the organs of secretion, that it resembles arterial blood. The fibrine coagulates firmly, and in some cases the serum which separates from it has a bright arterial colour, the colouring matter being not merely diffused through, but combined with, the serum. During the progress of this kind of fever the blood loses a large proportion of its fibrine and albumen, and becomes so thin that it oozes from the mucous membranes without any abrasion of surface, and in the last stage turns quite black from a diminution in the proportion of its salts.

Such are the appearances which the blood presents in the more severe fevers of hot climates. In this country, at the commencement or stage of depression the blood is dark and tarry, coagulates quickly, and forms a large clot with but little serum. As the stage of excitement advances, the blood becomes thinner and more florid, and flows more freely. Coagulation takes place more slowly, and a buffy crust is frequently formed on the surface of the clot. In the latter stage, when the powers are giving way, the blood becomes thinner, darker, and more dissolved. It scarcely coagulates at all, and is deficient in saline matter, and probably also in fibrine, thus nearly resembling menstrual blood, or the fluid mixture of serum and red particles, already mentioned as often found in the larger vessels after death. Such are the alterations which the blood usually undergoes in the different stages of simple continued fever, but in its more malignant forms, as in typhus, the blood is generally very watery, even from the commencement. As the disease advances, it gradually loses its power of coagulation, and in the last stage seems almost entirely deprived of fibrine.

Magendie has artificially produced an analogous state of blood by injecting putrid liquids into the veins of animals, and the speedily fatal disease which he thus caused had a strong analogy with typhus fever.[1]

To Dr. Stevens belongs the merit of having especially directed general attention to the circumstance that the saline matter of the blood gradually disappears in the progress of fever, and is almost entirely lost in its last stage. This he ascertained by direct experiment,[2] and his facts have since been confirmed by Jennings, who, in the interesting report already alluded to, gives an analysis of the blood in six cases of continued fever, in which the alkaline salts were found diminished in the following proportions :—

In healthy serum, according to Lecanu, salts	8.10
In the serum of a male, aged 31, first day of fever, salts . .	4
Ditto ditto aged 34, first day of fever, salts . .	5
Ditto female, aged 14, fourth day of fever, salts . .	4.2
Average of three other cases	4.4

[1] Journal de Physiologie, tom. iii. p. 83. [2] On the Blood, &c. p. 209.

Scurvy.—It seems to be the universal opinion of those who have seen and written on scurvy, that it owes its origin to a morbid change in the fluids, and especially in the blood; and even those who have been the most strenuous opposers of the humoral pathology in general, among the most celebrated of whom we may reckon Willis, Hoffman, Boerhaave, Cullen, and Sir John Pringle, have made an exception in favour of this disease. Notwithstanding this general belief, there has been no attempt up to the present time at any chemical examination of the properties of scorbutic blood, and we have only the general observation made by the surgeons of Lord Anson's expedition, (Messrs. Ettrick and Allen,) that in the beginning of the disease it flows from the arm in different shades of light and dark streaks; that, as this advances, it runs thin and black, and, after standing, turns thick and of a dark muddy colour, the surface in many places being of a greenish hue, without any regular separation of its parts; that in the third degree of the disease it is as black as ink, and, though kept stirring in the vessel for many hours, its fibrous parts have only the appearance of wool or hair floating in a muddy substance; and that in dissected bodies the blood in the veins is so fluid that by cutting any considerable branch, the part to which it belongs may be emptied of its black and yellow liquor, the extravasated blood being precisely of the same kind. The prevalence of scurvy where there has been a long continued use of salted provisions, has given rise to the supposition[1] that the salt itself actually finds its way into the circulation, and acts, as it is known to act on blood out of the body, by preventing its coagulation. This, however, is very evidently not the case, first, because salt provisions are not necessary to its production, since scurvy has often made its appearance where no salt provisions were used; as, for instance, in the Milbank Penitentiary, in 1819, where the diet consisted of pease, barley soup, and brown bread; and, secondly, because the appearance of the blood, especially as the disease advances, is exactly the reverse of what it would be on the addition of salt, which, instead of making it black, and causing it on standing to become thick, muddy, and of a greenish hue, would impart to it a fine scarlet tint that would remain permanent until it began to putrefy. Since the modern advances in animal chemistry, opportunities for examining the blood in true scurvy have been very rare; and it is therefore the more to be regretted that Drs. Latham and Roget, philosophers every way so competent to determine the precise morbid changes which it undergoes, did not, when they had it in their power, make a particular examination of it. Venesection, it seems, was practised at the penitentiary in a few cases, but nothing is stated respecting the appearance which the blood assumed.[2] The description of Lord Anson's surgeons does not by any means

[1] Jennings's Report.

[2] Account of the disease lately prevalent at the General Penitentiary, by P. M. Latham, M. D. 1823, p. 39.

apply to the blood which is found in purpura hæmorrhagica, a complaint that was, prior to the appearance of Dr. Bateman's work on diseases of the skin, generally considered closely allied to scurvy. In two cases of purpura related by Dr. Parry[1] of Bath, blood drawn from the arm exhibited a tenacious contracted coagulum covered with a thick coat of lymph; and in one instance which occurred under my care, where the patient, a man of forty-five years of age, had most of the symptoms of sea-scurvy, such as general cachexia, with anasarca of the lower limbs, great depression of spirits and prostration of strength, extensive ecchymosis on the trunk and the extremities, fetid breath and extravasations of blood from the gums, the stomach, and the bowels, as well as from a large foul ulcer on the leg; a copious venesection demonstrated that the blood had not in any degree lost its crasis, the crassamentum being covered with a thick buffy coat, and having as much firmness as is usual under the existence of such a state. It is proper to observe that Lind's description of the blood in scurvy differs from that of Lord Anson's surgeons, as he found it generally either natural or buffed.[2]

Jaundice.—In jaundice the blood, both arterial and venous, is tinged with bile, and this is apparent not only in the serum, but still more strikingly in the crassamentum, provided it be covered with a buffed surface. If this be removed and dried in a state of tension, it exhibits a deep yellow hue, particularly when viewed by transmitted light. Although the bile is thus rendered very visible in jaundiced blood, yet, owing to its combination with albumen, which defends it from the action of acids, it is difficult of detection by chemical re-agents, so that many chemists of eminence have sought in vain to ascertain its presence. Lassaigne, however, succeeded in demonstrating that the colouring matter of the bile is really to be found in the circulation, and Berzelius tells us that Collard and Martigny pretended to have discovered even the resin of bile in jaundiced blood. M. Lecanu has more recently confirmed these facts, and Mr. Kane has verified his results.[3] To the medical enquirer who does not follow the minutiæ of animal chemistry, the identity of the colouring matter in the serum of jaundiced blood with that of the bile itself will be rendered sufficiently evident by adding to it an equal quantity of sulphuric acid diluted with twice its bulk of water. The serum will thus change its yellow hue for the characteristic green colour of acid bile. Experimentalists have failed in producing this effect, being probably misled by having found that the small proportion of acid which is required to strike a green colour with urine charged with bile, produces no such effect when added to jaundiced serum.

Disease of the Kidney.—In those organic diseases of the kidney which are characterised by anasarca and the passing of urine

[1] Edinburgh Medical and Surgical Journal, vol. v. p. 7.
[2] Lind on Scurvy, p. 512.
[3] Dublin Journal, vol. ii. p. 346.

coagulable by heat and acids, the albumen of the blood is more or less deficient in proportion; and this is marked by a corresponding diminution in the specific gravity of the serum. In a letter to Dr. Bright, published in the first volume of that author's Reports of Medical Cases, page 83, Dr. Bostock states, in reference to the blood in these diseases, that the crassamentum was for the most part covered with a thick buffy coat, and was generally of a firm consistence. The appearance of the serum was more varied. It was occasionally turbid, and upon standing for twenty-four hours a white creamy substance rose to the surface; but no proper oily matter could be detected in it. On exposing it to heat, it coagulated in the ordinary manner, except that the coagulum seemed to contain an unusual number of cells, and that a greater quantity of serosity separated from it. "I think I may venture to say," adds the writer, "that the serum generally in these cases contained less albumen than in health, although I am not able to state precisely the amount of this difference. The serosity which drained from the coagulated albumen on being evaporated was found to consist in part of an animal matter possessing peculiar properties, which seemed to approach to those of urea; it was partially soluble in alcohol, and was acted upon in a somewhat similar manner by nitric acid."

The above remarks were made on specimens of blood furnished from time to time by Dr. Bright. The number is not stated, nor was the specific gravity of the serum taken. Dr. Bostock gives a case, however, (page 85,) where, after stating that the crassamentum was remarkably buffed and cupped, he adds, "The serum was also worthy of attention, as taken in connection with the state of the other fluids. Its specific gravity was almost exactly the same with that of the urine, being no more than 1.013, which I believe to be lower than had ever occurred to me in the numerous experiments which I have made upon this substance. We have here, therefore, an example of blood exhibiting a very great deficiency of albumen, at the same time that we observe the mode in which it passes off from the system by means of the kidney, while this organ has its appropriate office of secreting urea nearly suspended. I regret that I did not attend particularly to the specific gravity of the other specimens of dropsical serum which you sent me. From some incidental remarks in my notes, I suspect that its specific gravity would have been found lower than ordinary; but it is a circumstance which I shall be anxious to ascertain when any opportunity occurs." This suspicion is completely confirmed by other cases that have occurred to myself, in which the fact was also established beyond doubt, that the animal matter found by Dr. Bostock in the serosity was not merely an approach to urea, but that principle itself possessing all its usual characters. The following may serve as an example of light serum.

William Squires, aged 54, labouring under organic disease of the kidneys and chronic bronchites with anasarca, had for many

months voided urine which coagulated on the application of heat or the addition of nitric acid.

The specific gravity of his blood at 88° Fahr. was 1.041
Do. Serum at 68 . 1.021
healthy standard 1.030.
This blood contained in 1000 parts,
3.845 fibrine:
healthy standard 2.1 to 3.56
55.000 albumen:
healthy standard 65 to 69.

In this case 100 grains of urine contained 6.666 albumen. There was consequently nearly one eighth as much albumen in the urine as in the blood, and the patient lost as much of that constituent daily as if he had been bled to the extent of four ounces.

The following cases are from notes with which I have been favoured by Dr. G. H. Barlow, who has devoted much attention to the examination of the blood and urine in this disease.

No. 1. A patient affected with general anasarca—Urine copious, clear, pale, coagulable by heat and nitric acid: specific gravity 1.011. Blood cupped and buffed, serum milky: specific gravity 1.019.

No. 2. Man aged 48, anasarcous—Urine dingy brown, natural in quantity, acid, coagulable; specific gravity 1.017, contained 4½ per cent. of albumen. Serum of the blood, specific gravity 1.013.

No. 3. A man who was found on post-mortem examination to have granulated kidneys—Urine reddish brown, very scanty, coagulable; specific gravity 1.008. Blood cupped and buffed; specific gravity (of the whole blood) 1.037.

In my paper on the blood in the Medico-Chirurgical Transactions, vol. xvi., I have stated the case of a woman forty-eight years of age, who for ten weeks had complained of pains in her loins, anasarcous swelling of her legs, and general debility, and who passed urine which was in a high degree coagulable. I examined her blood, and found it to contain 0.43 per cent. of fibrine, and only 1.61 per cent. of albumen. The specific gravity of the serum was 1.020 at 60° Fahr. In that paper I have also observed that in several cases marked by coagulable urine, I have examined the specific gravity of serum with which Dr. Bright has furnished me, and have always found it much below the healthy standard.

It is not, however, in this complaint exclusively that the albumen of the blood will be found deficient in proportion. In other dropsical affections it will sometimes happen that a proportion of albumen more than equivalent to the fibrine effused will disappear from the circulation. Eleven days after tapping a young woman, in whom ascites had supervened upon rheumatic affection of the heart, she was observed to be filling again very fast. A few ounces of blood were taken from the arm, and this blood was found to contain 0.319 per cent. of fibrine, and only 3.51 per cent. of albumen. Her serum had a specific gravity of 1.023.

The experiments of MM. Prevost and Dumas (Annales de Chimie, vol. xxiii.) which have since been repeated by Gmelin and Tiedemann (Poggendorff's Annalen), prove satisfactorily that urea exists in the blood after the kidneys have been extirpated, and consequently that it is not formed, but merely abstracted, by those organs. So long, however, as the kidneys act, we cannot expect to find it, since it is removed from the circulation as fast as it is formed, and never exists in any considerable quantity.

In these cases of diseased kidney a result analogous to that which follows extirpation occurs; for while that organ is permitting albumen to pass through it unchanged, the urea which it should separate is very generally, if not always, found in the blood. This I have proved in repeated instances, and it is now so generally admitted, from the experiments of Prout, Christison, and others, that it is scarcely worth while to cite cases. Dr. Bright, vol. ii. p. 447, alludes to several specimens of serum from patients under this disease, which he had sent me for examination, in some of which I did, and in others I could not, detect urea. In one very remarkable instance of a young woman, the albuminous state of whose urine constantly existed for above three years, the urine contained less than one third of the normal proportion of urea, while about one per cent. of albumen supplied the deficiency. The serum of the blood was, as I have already remarked to be usual in this disease, of very low specific gravity, being only 1.021. The quantity of albumen in 1000 grains amounted, after careful drying, to only fifty grains instead of seventy-eight (Lecanu's healthy standard), and it contained fully as much urea as the urine itself—the 1000 grains yielding nearly fifteen grains of that principle.

It may not be out of place here to observe, that in this disease not only does the blood itself contain urea, but all those effusions also which are formed from it, and which take place in the different serous cavities. I have repeatedly detected urea in these cases in the serous effusion into the ventricles of the brain; and Dr. Barlow found it in one case, 1st, in abundance in the ventricles of the brain; 2dly, scantily in the effusion into the pleura and pericardium; and 3dly, in abundance in the peritoneum. In a second case of a similar nature urea was obtained in abundance from the fluid of the pericardium. In a third, the effusion collected after death from the pleura of a man who had suffered from general dropsy and mottled kidney yielded a very satisfactory specimen of urea.

I have dwelt at some length on this subject, as it is only of late years that the attention of the medical world has been drawn to it through the writings of Dr. Bright, and still more recently that the morbid changes presented in the blood have been investigated.

Diabetes.—In this complaint the blood unquestionably undergoes some material change, although its nature has not hitherto been very successfully investigated. This may be inferred from the great length of time during which it is capable of resisting putrefaction—a circumstance first noticed by Rollo, and which,

though doubted by some authors, I have had opportunities of confirming in several instances. Nicolas and Gieudeville[1] have observed that it contains an increase of serum and very little fibrine; but this is not borne out by my own experience as deduced from many specimens of diabetic blood which I have examined, neither can its antiseptic qualities be attributed to any deficiency in the proportion of azote; for Dr. Prout, who has made accurate experiments to determine this point, has found it not to differ in this respect from the standard of health. The most eminent chemists, both abroad and in this country, have endeavoured in vain to detect sugar in diabetic blood. Dr. Wollaston ascertained that the smallest portion of saccharine matter, added to serum previously to its coagulation by heat, prevents the subsequent crystalisation of the salts it contains, yet that in diabetic serum those salts crystalised with the same facility as in that procured from a person in health. The same reasoning as that which has been adduced to prove that urea may be formed in the blood, although it is not to be detected there while the kidneys perform their office, will also apply to the existence of sugar in the blood of those affected with this disease. I am not aware that the arterial blood has been made the subject of experiment, and yet it is possible that it might exist in the arteries alone—for we have only to suppose it to enter the circulation with the chyle, and after having been carried through the lungs, the left cavities of the heart, and the aorta, to be again withdrawn from the circulation by the kidneys. I do not pretend, however, that this supposition carries with it any degree of probability.

Cholera.—There is no disease in which the blood undergoes more remarkable changes than in malignant cholera; not indeed in the incipient stage, as affirmed by Dr. Stevens, but in direct proportion to the intensity and duration of the collapse. In appearance it is thick and dark, bearing a strong resemblance to treacle or tar. It is of high specific gravity, the serum varying from 1.040 to 1.045, at 60° Fahr.; and, according to M. Lecanu, the solid matter which it contains is sometimes double that of the healthy proportion. Most of its physical characters are satisfactorily accounted for by its analysis, which has been accurately made by several eminent chemists, among whom we may mention Dr. Turner, M. Lecanu, and Dr. O'Shaughnessy. Cholera blood, according to these authorities, contains less water and more albumen and hæmatosine than healthy blood, and its salts are in unusually small quantity, or almost entirely wanting. Dr. O'Shaughnessy, who has detected urea in cholera blood, states that the summary of his experiments denotes a great but variable deficiency of water in the blood of four malignant cholera cases; a total absence of carbonate of soda in two; its occurrence in an almost infinitesimally small proportion in one, and a remarkable diminution of the other

[1] Annales de Chimie, vol. xliv. p. 69.

saline ingredients; lastly, the microscopic structure of the blood, and its capability of aeration, are shown to be preserved. The cause of the dark colour of the blood in cholera is a point which we are told by Dr. Turner is by no means decided. Dr. Thomson and Dr. O'Shaughnessy are at variance on the question of its susceptibility of arterialisation. Dr. Stevens rather unphilosophically makes its dark colour to depend *primarily* on the poisonous cause of contagion, yet attributes it also to a deficiency in the proportion of saline matter. It is probably not owing to either of these causes, but to a defective circulation through the lungs, from which the blue livid tint frequently observed over the surface of the limbs likewise originates. The corresponding diminution of animal heat gives countenance to this supposition.

Chlorosis.—Among other changes which occur in the progress of chlorosis, there is none more constant than an impoverished condition of the blood, which is thin, light-coloured, and weakly coagulable—being deficient in fibrine, and still more so in the proportion of the red particles. To the latter cause is to be attributed the diminished temperature of the surface, together with the universal pallor and waxy appearance which those who are the subjects of this disease generally exhibit. The deficiency of colour in the catamenia, and the pale stain which hemorrhages from the nose leave on linen, are also referable to the same cause. In aggravated cases, if blood be drawn from the arm, the crassamentum is observed to be of a pale rose colour, and small in proportion to the serum. We have to regret that in this, as in most other cases of morbid blood, pathologists have contented themselves with a general observation of facts, without attempting to investigate them with that degree of precision which can alone lead to a further advancement of our knowledge respecting their causes. The only analyses of chlorotic blood of which I can find a record are given by Mr. Jenkins in two well-marked cases of chlorosis; the one of a girl aged fifteen, the other of a young woman aged twenty-one. In these the blood contained 871 and 852 parts in a thousand, of water, respectively, instead of 780, the healthy standard; and the colouring matter amounted to 48.7 and 52, instead of 133. The albumen and salts were in the usual proportions.

Melanosis.—Although it would be foreign to my present object to treat of the various morbid products which may be supposed to have their origin in a diseased state of the blood, yet there is one which seems so evidently to be the result of an accidental change in that fluid, that it must not be passed over without a brief notice. The similarity of chemical composition in the blood, and in the matter of melanosis, is such as to leave little doubt that the material of which the latter is composed has its origin in the circulation, and is afterwards deposited in the various parts in which it is found. The different analyses of melanosis, says Andral, all concur in one important point. They all show that the accidental production called melanosis is formed of the different elements of

blood, and especially of a colouring matter which more or less resembles that of the blood, but which is, nevertheless, not identical with it. M. Foy, in his analysis, calls this altered cruor. Dr. Carswell, to whom we owe the most detailed and best account of melanosis which we possess, states that he has fixed its seat in the blood, not only because it is seen there, but because his anatomical researches show that it is there formed. He makes a grand division of melanosis into true and spurious; the former of which occasionally makes its appearance in the circulating system—a fact which is well established—while the latter is more decidedly the result of chemical action. Whenever healthy blood comes in contact with an acid, whether in or out of the body, its colour changes from red to brown or black, in proportion to the strength and abundance of the acid employed. It is to this cause that we are to attribute the appearance of brown or black ramifications, patches, or points, as observed after death in the stomach and intestines. To this cause also are owing the accumulations, during life, of black pitchy matters in the alimentary canal; and it is by the acidity of the black vomit, and its power of reddening litmus paper, as we learn from Dr. Stevens, that it can alone be distinguished from blood rendered black by defective decarbonisation or the absence of saline ingredients. Where a hemorrhage occurs, whether by the rupture of a large vessel or by a general oozing from the mucous membrane into the stomach or bowels, we shall find the fluid ejected assume the appearance of red blood or of brown or black matter, according to the presence or absence of the gastric juice in an acid state. Upon this almost accidental circumstance, then, will it depend whether we are to designate the disease hæmatemesis or melæna, there being in reality no essential difference between the two diseases. The black discoloration of blood, which occurs whenever it becomes stagnant from retarded or interrupted circulation, will, by those who follow the views of Dr. Stevens, be attributed to a similar cause. According to that author, it is the presence of carbonic acid which acts like other acids in rendering venous blood dark, and it is its abstraction by oxygen which, combined with the action of the saline matters it contains, restores it to its scarlet hue.

The foregoing are among the more prominent diseases in which the blood has been observed to undergo changes either directly cognisable by our senses, or discoverable by those chemical and mechanical means which we are enabled to call to our assistance. There are, however, other morbid conditions the existence of which is equally certain, although their essence is of such a doubtful nature that it defies detection by the coarse instruments and the limited skill which man, in the present state of his knowledge, is enabled to employ. In the exanthematous diseases the blood partakes of the general disorder of the system. Dr. Home, of Edinburgh,[1] succeeded in reproducing measles by inoculation with

[1] Duncan's Medical Commentaries, vol. xix. p. 213.

blood drawn from a superficial vein in one of the patches of eruption which cover the skin in that disorder; and though others have failed in this experiment, it has been successfully and often repeated by Professor Speranza of Mantua. Pregnant females affected with small-pox, or even exposed to its virus, though they may have had the disease, have often imparted it to the fœtus in utero,[1] and syphilis has been communicated in the same manner. Professor Coleman has proved by experiment that the blood of a glandered horse will impart glanders if infused into the veins of a healthy animal. Dupuy and Leuret have thus produced malignant pustule; transfusion of the blood of a mangy dog has produced mange in another; and, according to Dr. Hertwich, of Berlin, the blood of a rabid animal will by inoculation communicate the disease. A remarkable instance is related by Duhamel, in which a butcher became affected with a malignant pustular disease, in consequence of having put into his mouth the knife with which he had slaughtered an over-driven ox. Another individual lost his life from sphacelus of the arm, in consequence of a wound in the palm of his hand, accidentally inflicted by a bone of the same animal; and in two women who received some drops of its blood —the one on her hand, the other on her cheek—inflammations ensued which rapidly terminated in gangrene.

Although in all these instances there can be no doubt that the blood was in a poisonous state, there is no reason to suppose that this could have been foretold by any thing remarkable in its appearance or sensible qualities. Scarcely more successful in general has been the search for extraneous poisons, which nevertheless have appeared, from collateral circumstances, to have entered the circulation, or have even been purposely introduced into it. Dr. Christison[2] has cited a sufficient number of cases, where poisons swallowed have been afterwards found in the blood, to show that we must not infer their absence from our inability in most cases to abstract them in a separate form; and he further demonstrates how erroneous such an inference might be by stating that Dr. Coindet and himself, after destroying a dog in thirty seconds by injecting eight and a half grains of oxalic acid into the femoral vein, endeavoured in vain to detect any portion of it in the blood of the iliac vein and vena cava collected immediately after death, although it is highly improbable that it could have passed off by any of the secretions in so short a time.

The chief obstacles by which we are opposed in such researches are minuteness of quantity and decomposition. When only a few grains of a poison are absorbed, and thence diffused not only through the whole mass of circulating blood, but likewise among all the various tissues and solids of the body—being moreover carried off by the kidneys, perhaps nearly as fast as they enter by

[1] Edinb. Med. and Surg. Journ. for April, 1807. Med. Chir. Trans., vol. i. p. 272.

[2] Christison on Poisons, p. 14.

the circulation—it cannot be matter of surprise, however delicate our tests may be, that they are seldom to be met with even where still retaining their chemical characters. When we consider, however, that re-agents, which produce a change of properties in those bodies with which they are brought in contact, do probably themselves undergo a corresponding change, we shall readily perceive that our difficulties will be still further increased on this account.

The products of diseased action, and especially pus, have been often met with, as well in the arteries and veins as in the cavities of the heart; but it yet remains a matter of doubt whether these are actually formed in the blood, or whether, as seems to me more probable, they are not rather carried into the circulation from other parts in a degenerate or diseased state, or are the products of inflammation in the lining membrane of the blood-vessels themselves.

With respect to those cases where worms and insects are said to have appeared in the blood, whereof many are recorded, some are referable to the head of false polypi, the shape of which has misled the observer—others, to deception or the accidental presence of insects or their ova in the receiving vessel; and though we cannot deny the possibility that parasitical animals may exist in the fluids as well as in the closed cavities and solids of the body, yet we require better evidence than has yet been adduced to confirm our belief in the existence of entozoa in the circulating current. In a recent case brought forward by Mr. Bushman,[1] and learnedly illustrated by that gentleman, it would, I confess, have carried more conviction to my mind, had he himself watched the blood from the moment of its quitting the vein until the larvæ which he describes were seen swimming in its serum. In such extraordinary cases, the mind is not satisfied with any thing short of moral certainty.

From what has been set forth in the foregoing pages, it will be perceived that our knowledge on the subject discussed in them yet remains extremely defective. We learn, indeed, that under the existence of disease the different constituents of the blood are liable to morbid increase or diminution as well as to certain alterations in their sensible qualities, hitherto less accurately examined; that there are instances in which principles not usually met with in the healthy circulation may be detected in it, and others where those which are always present in a state of health do nearly if not altogether disappear. But that which still remains unknown, and to which it is of the highest interest and importance that our investigations should be directed, is the connection that these morbid changes have with the diseases which they accompany; the position which they occupy in the relation between cause and effect. Perhaps our present information is not sufficiently minute to give fair expectations of any considerable advances being made in this line of enquiry; for, when we contemplate the variety of

[1] History of a case in which animals were found in blood, &c.

materials for the formation and removal of morbid as well as of healthy secretions and structures, which are stealing unperceived along the vital current, we are forced to confess how small is the sum of all we know compared with that of which we are still ignorant, and how ample is the harvest which yet remains to be gathered by future labourers in this field of research.

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LECTURES

ILLUSTRATIVE OF

CERTAIN LOCAL NERVOUS AFFECTIONS.

BY SIR BENJAMIN C. BRODIE, BART., F. R. S.
Sergeant Surgeon to the King, and Surgeon to Saint George's Hospital.

ADVERTISEMENT.

In these lectures, which are now offered to the public in a separate form, having already appeared in a periodical publication, it has been my object, not to give a complete history of any one disease, but to portray a certain order of symptoms, which, while they have many characters in common, may arise from various causes, and require very different modes of treatment for their relief.

It seems to me that this is a method of illustrating subjects in practical surgery, which might he adopted much more frequently than it is, with the greatest advantage to the younger members of our profession. In the systematic form which the writings of pathologists usually assume, it is impossible to find a place for a large proportion of that knowledge which long experience has enabled physicians and surgeons to obtain, and to receive which at an early period must be of the first importance to those who are following them in their professional career. This defect would be in great measure obviated, if writers would sometimes condescend to treat of symptoms rather than of diseases; not, of course, to the exclusion of the latter, which would be absurd; but taking a particular symptom or order of symptoms as the basis of their enquiries, and referring them to the various diseases from which they may arise; instead of confining themselves to the history of a particular disease, and of the symptoms by which it is indicated.

LECTURE I.

Theory of local nervous affections—Various circumstances under which they exist—Principles on which the treatment of them should be conducted.

A middle-aged lady, who had been exposed during a considerable period of time to the operation of causes of great mental anxiety, complained of a constant and severe pain, which she referred to a spot, about three or four inches in diameter, in the situation of the false ribs of the left side. Besides this she was subject to fits, apparently connected with hysteria, and was otherwise in a very impaired state of health. Under these circumstances she died, and on examining the body after death, particular attention was paid to the side to which the pain had been referred. No morbid appearances could be detected in it; there was neither inflammation, nor thickening, nor adhesion, nor any morbid change

of structure, nor the slightest deviation of any kind from the natural condition of the part.

Now such a case as this is by no means uncommon. It is only one of many which might be adduced in proof of this proposition, namely, that the natural sensations of a part may be increased, diminished, or otherwise perverted, although no disease exists in it which our senses are able to detect either before or after death.

There are other cases which may be regarded as corresponding to those to which I have just alluded, except that the nerves of motion are affected instead of those of sensation. Here there is an involuntary contraction or spasm of a particular set of muscles, or certain muscles lose their power of action altogether, and become paralytic; and yet, if an opportunity occurs of examining the parts after death, the most minute dissection can demonstrate nothing in them different from what there would have been if the spasm or paralysis never had existed.

Nor are these facts of difficult explanation. Every part, to which a nervous filament can be traced, may be said to have its corresponding point in the brain or spinal marrow, and an impression made either at its origin, or any where in the course of the trunk of a nerve, will produce effects which are rendered manifest where the nerve terminates, at that extremity of it which is most distant from the brain.

These local nervous affections are of very frequent occurrence. In one shape or another you will meet with them at every turn of your future practice, and a knowledge of them is of the greatest importance, both to the physician and surgeon. Without it you will be continually mistaking the real seat of a disease: your attention will be directed to a wrong object, and, following the symptoms, you will be in danger of overlooking the cause on which they depend. The investigation, however, is not unattended with difficulty, and it will often require all your professional sagacity and skill to trace the phenomena, which occur in these cases, to their true origin.

If you accidentally strike the inside of your elbow against a projecting body, the corner of a table, for example, you feel a peculiar tingling sensation, not where the blow is inflicted, but where the ulnar nerve, which has been struck, terminates, on the inside of the hand, and especially in the little finger. In like manner, an accidental pressure made for a few minutes on the popliteal or sciatic nerve, will cause that peculiar tingling sensation in the foot which is commonly described by saying that the foot is asleep, and which continues for some time after the pressure has been taken away. Guided by the light of these facts, and of others analogous to them, the first question which you will ask yourselves when you are consulted in these cases, will be, whether there is any cause of irritation affecting the trunk of the nerve above, sufficient to account for the symptoms which are met with in the part to which its ultimate fibres are distributed?

A man was admitted into St. George's Hospital in the year 1808,

complaining of a severe pain in the inside of his knee. The joint was carefully examined, but no marks of disease could be detected in it. In the thigh, however, there was an aneurism of the femoral artery, of the size of a small orange. This last disease had scarcely attracted the patient's notice. He said that he should be very well if it were not for the pain in the knee, and it was not until some trouble had been taken to explain to him the nature of his case, that he could be made to understand that the tumour was of any importance. Soon after the man's admission, Sir Everard Home (then Mr. Home) applied a ligature round the femoral artery, in the upper part of the thigh. On the instant of the artery being secured, the tumour ceased to pulsate, and the pain in the knee ceased also. Some untoward circumstances occurred, and the patient died about four or five days after the operation was performed. On inspecting the limb after death, the aneurism was found reduced to one half of its former size; some branches of the anterior crural nerve, which passed over it, and which must have been kept on the stretch previous to the operation, were found to terminate in the part to which the pain had been referred, on the inside of the knee; and thus the cause of the pain was sufficiently explained. It was, in fact, a nervous pain, existing where there was no disease, in consequence of pressure on the nerves above.

A gentleman in the year 1816 began to suffer from a gnawing pain in the left leg, referred to the course of the peroneal nerve from the foot to the knee. The pain by degrees became very severe, occupying at the same time a larger portion of the limb. The limb itself presented no appearance of disease. The patient consulted various surgeons, myself among the number. The disease went by the name of neuralgia, but the cause of it could not be discovered, and the remedies recommended were of no avail. After having lost sight of him for a considerable time, I was again sent for to see him in the year 1824. He was now dying with dropsy of the belly, and anasarca of the lower limbs. On examining the abdomen, it was observed, as the fluid which it contained receded under the pressure of the hand, that there was a large solid tumour attached to the left side of the lumbar vertebræ, and extending into the pelvis. It was evident that this tumour must have pressed on the origin of the sciatic nerve, and thus it afforded a sufficient explanation of the pain which for so many years had been referred to some of its branches.

A case analogous to this is recorded by Dr. Denmark, in one of the volumes of the Medico-Chirurgical Transactions. A sailor received a wound from a musket-ball in the arm: the wound healed, but the patient complained of an agonising pain, beginning in the extremities of the thumb and fingers (except the little one), and extending up the fore-arm. His sufferings were such that he willingly submitted to the amputation of the limb, and the operation gave him complete relief. On dissecting the amputated limb, a small portion of lead, which seemed to have been detached from

the ball when it had struck against the bone, was found imbedded in the fibres of the median nerve.

In each of these cases the cause of irritation was detected in the trunk of the nerve belonging to the part to which the symptoms were referred. But similar effects are produced where the actual seat of the disease is in that more essential part of the nervous system in which the nerve itself originates; that is, in the brain or spinal marrow. Thus, caries of the dorsal vertebræ, irritating the spinal marrow, produces pains and muscular spasms of the lower limbs; and the same disease, affecting the superior cervical vertebræ, produces corresponding symptoms in the upper limbs.

A gentleman complained of severe pains referred to one side of the abdomen. After having been fixed for some time in one situation, it attacked another. No disease could be detected in the part apparently affected, and the pains were therefore regarded as nervous. It was observed at the same time that his powers of articulation were affected, and that he spoke in an indistinct and drawling manner. This seemed to indicate that there was some disease in the brain, and the suspicion was confirmed soon afterwards by the occurrence of epileptic fits, from which the patient continued to suffer during the few remaining years of his life.

I mention this case because I believe that a particular example will serve to impress the fact, which it illustrates, on your minds better than a mere general observation, and not because there is any thing in it in any way remarkable or singular. You will, indeed, when engaged in practice, find nothing more common than this: that a patient consults you who labours under some disease in the brain, but in whom a particular symptom, referred perhaps to a distant part of the body, is so severe, or so distressing, that he regards this as the original disease; and it is only after a diligent cross-examination that you are enabled to detect the existence of those other symptoms which serve to explain the real nature of the case. In many of these cases the cause of irritation seems to operate always on the same part of the sensorium, and there is little or no variety in the local indications by which it is rendered manifest. At other times it has no determined seat; it may affect at first one portion of the brain, to which a certain function belongs, and then it may affect another portion, whose function is entirely different, and the symptoms vary accordingly.

A gentleman laboured under a most severe pain, referred to the left side of the face, to which those whom he consulted gave the name of tic douloureux. While under the influence of this pain he was suddenly seized with a pain in the calf of the left leg, having precisely the same character with that which he had before experienced in the face. When the pain in the leg attacked him, that in the face did not subside altogether, but it abated so much that he suffered little or no inconvenience from it. At the end of a few days, as the pain left the leg, it returned with its usual severity in the face.

A lady became affected with a spasmodic affection of the sterno-cleido mastoideus muscle, producing what is commonly called a spasmodic wry neck. This symptom continued unabated for a year, and then suddenly left her; but, as the spasm in the muscle ceased, she fell into a state of mental depression amounting to insanity, and in this she continued during the whole of the second year. At the end of this period she recovered of the disordered condition of her mind, and the spasm of the muscle returned, continuing from that period up to the time of my being consulted—three or four years afterwards. I was consulted by another lady, in whom a neuralgic affection of the spine alternated with insanity.

When a calculus passes along the ureter from the kidney into the bladder, it frequently occasions a severe pain in the testicle of the same side. The most probable explanation of this sympathetic affection of the testicle is as follows: many of the nerves of the testicle derive their origin from the renal plexus, which also supplies the kidney, and which is formed by branches of the great sympathetic nerve. The irritating cause—namely, the calculus—operates in the first instance on the nerves of the kidney, through which its influence is transmitted to the renal plexus; and from thence it is, as it were, reflected to the nerves of the testicle.

The symptoms which occurred in the following case may be accounted for on the same principle. A gentleman laboured under a scrofulous disease of the hip, producing caries of the bones and suppuration within the joint. The following symptoms existed in addition to those which the same disease usually produces. The smallest motion of the thigh induced an attack of excruciating pain, amounting to agony, attended with violent spasmodic contraction of the muscles which move the thigh. The limb was jerked in a most remarkable manner for several minutes, and the volition of the patient had no control over these distressing and extraordinary movements. After some time a tumour began to present itself externally on the anterior part of the limb, raising the femoral artery which lay pulsating on its surface. Combined with the disease of the hip joint there were scrofulous tubercles and abscesses of the lungs; and of this last-mentioned disease he died, the attacks of pain and spasm having subsided for six or eight weeks before this event took place. Having the opportunity of doing so, I did not fail to examine the diseased hip, and the parts connected with it, with the greatest care. The bones composing it were soft, so that they were readily divided with a scalpel; the cancelli contained a yellow cheesy matter, and the cartilages had been destroyed by ulceration. The tumour was formed by an abscess situated among the muscles of the thigh, on the anterior part below the hip joint, but communicating with it. Two lymphatic glands, enlarged to the size of large walnuts, were found situated beneath the skin on the anterior part of the thigh, below the outer extremity of Poupart's ligament. It so happened that a considerable branch of the lumbar nerves lay over each of these enlarged glands—being thus kept stretched and tense in the same

manner as the strings of a violin are stretched over the bridge of the instrument. These nerves had the same origin with those which supply the muscles on the anterior and inner part of the thigh, and the peculiar circumstances under which they were placed seem to afford a sufficient explanation of the peculiar symptoms under which the patient laboured. Nor is the view of the case different if we refer the symptoms to the pressure of the abscess, since this affected the nerves partially; whereas the convulsive action of the muscles was general, and the psoas magnus muscle, which was situated above the abscess, was not less liable to spasm than those which were situated below.

In cases similar to that which I have just mentioned, where nerves have a common origin, it is easy to suppose that an impression made upon one nerve should be communicated to those parts which are supplied by the other. But an impression made on one part of the body will often produce a nervous affection elsewhere, at a distance from the original seat of the disease, and where no such obvious explanation of the fact presents itself. A disease in the liver produces a pain in the right shoulder; a disease in the heart produces a pain in the back.

A gentleman awoke in the middle of the night, labouring under a severe pain in one foot; at the same time that some other sensations, to which he was not unaccustomed, indicated the existence of an unusual quantity of acid in the stomach. To relieve the latter, he swallowed a large dose of an alkaline medicine. Immediately on the acid in the stomach having been thus neutralised, the pain in the foot left him.

The late Dr. Wollaston was accustomed to relate the following history:—He ate some ice-cream after dinner, which his stomach seemed to be incapable of digesting. Some time afterwards, when he had left the dinner-table to go to the drawing-room, he found himself lame from a violent pain in one ankle. Suddenly he became sick; the ice-cream was rejected from the stomach; and this was followed by an instantaneous relief of the pain in the foot.

A gentleman consulted me concerning a pain in one instep. The pain was severe, causing lameness, so that he walked with difficulty; but there was neither swelling, nor, except the pain, any mark of inflammation. I prescribed some remedies, which, however, were of no avail. One morning he called on me, still suffering from the pain in the foot, and so lame that he could not get out of his carriage and walk into the house without the assistance of his servant. Now, however, he complained of another symptom; he had a difficulty of making water, and a purulent discharge from the urethra. He had laboured under a stricture of the urethra for many years, and had occasionally used bougies. Of late the stricture had caused more inconvenience than usual; but he had abstained from mentioning it, thinking that it would be better that he should (if possible) be relieved of the pain in the foot before any treatment was adopted on account of the stricture. Under these circumstances I introduced a bougie, which penetrated the

stricture and entered the bladder. Immediately on the bougie having been used, the pain in the foot abated; and in less than a quarter of an hour he left the house free from pain, and walking without the slightest difficulty. This happened some years ago, but I have seen the patient at intervals ever since; and, from a most careful observation of his case, he and I are both satisfied that the pain in the foot is connected with the disease in the urethra, and we have never found any thing to relieve it except the introduction of the bougie.

A lady consulted me concerning a pain to which she had been for some time subject, beginning in the left ankle, and extending along the instep towards the little toe, and also into the sole of the foot. The pain was described as being very severe; it was unattended by swelling or redness of the skin, but the foot was tender. She laboured also under internal piles, which protruded when she was at the water-closet, at the same time that she lost from them sometimes a large and sometimes a smaller quantity of blood. On a more particular enquiry, I learned that she was free from pain in the foot in the morning; that the pain attacked her as soon as the first evacuation of the bowels had occasioned a protrusion of the piles; that it was especially induced by an evacuation of hard fæces; and that, if she passed a day without any evacuation at all, the pain in the foot never troubled her. Having taken all these facts into consideration, I prescribed for her the daily use of a lavement of cold water; that she should take the Ward's paste (*confectio piperis composita*) three times daily, and some lenitive electuary at bed-time. After having persevered in this plan for the space of six weeks, she called on me again. The piles had now ceased to bleed, and in other respects gave her scarcely any inconvenience. The pain in the foot had entirely left her. She observed that, in proportion as the symptoms produced by the piles had abated, the pain in the foot had abated also.

Now, in such cases as these, you will at once perceive that there is no direct communication between the nerves of the parts affected that will afford a reasonable explanation of the occurrence of the sympathetic pain; and you will naturally enquire, how then is the sympathetic pain produced? To this question I would answer, that in all probability it is in the brain itself that the communication is made—the impression being first transmitted to the sensorium, and from thence reflected to the nerves of the part which is secondarily affected. If you dissect the brain according to Reil's method, having first hardened it by maceration in alcohol, you will find it splitting into fibres, passing in various directions, many of which may be demonstrated as connecting even the most distant convolutions of the cerebrum with each other; and if, with the limited knowledge which we at present possess, we venture to speculate on this obscure but interesting subject, we may easily be led to suppose that an impression on one part of the body should, by means of these communicating fibres, produce a disordered sensation in another part. It is not more improbable that this should

happen, than it is that the whole fabric of the nervous system should sympathise with an affection of a particular nerve, as is the case in traumatic tetanus, and on many other occasions of which the experience of surgeons will furnish numerous instances. I shall mention here one remarkable example of the kind which fell under my observation. An officer in the army received a wound (in action) from a musket-ball in the leg. The wound healed, but the ball remained lodged in the flesh, in some deep-seated situation where it could not be felt externally, and giving the patient no inconvenience. After some time the ball changed its place, so that it became perceptible to the touch; but in its new position it occasioned symptoms such as had never existed previously. There were convulsive twitches of the muscles of the limb, occurring at irregular periods, and sometimes followed by a fit, in which there were general convulsions, as in epilepsy. At this time (if I may judge from the patient's own account) the ball might have been readily extracted. Unfortunately the opportunity was neglected, and soon afterwards the ball again shifted its place. Probably it went back to the situation which it had originally occupied; at any rate the spasms of the muscles were relieved, and there was no recurrence of the epileptic fits. I presume that these latter symptoms were the consequence of the ball, when it had left its original position, pressing on some nervous filament in such a manner as that a peculiar irritation was excited in it and transmitted to the brain.

As these nervous affections may occur under such different circumstances, and may arise from such different causes, you will not be surprised to find that they assume a great variety of characters, so that it is impossible for me to do more than give you a general notion of what you will observe respecting them in the course of your professional practice; your own experience will enable you hereafter to supply the deficiencies of my description.

One remarkable feature of these diseases, whether they present themselves in the form of nervous pains or muscular spasms, is, that they seem to be suspended during sleep. A patient, suffering from the pains of tic douloureux in the face, may, for a time, be prevented falling asleep, but, if once asleep, his sleep is likely to be sound and uninterrupted for many hours. In like manner, when a patient is affected with the spasmodic wry neck, the muscle which is the seat of the spasm, probably the sterno-cleido-mastoideus, becomes relaxed, and remains so while sleep continues, perhaps during the whole night. I do not assert that there are absolutely no exceptions to this rule, but I am much mistaken if the exceptions are not comparatively rare. Even during his waking hours, the sufferings of the patient are seldom constant. Nervous pains especially are intermissions, occurring in paroxysms, and then either subsiding altogether, or becoming very much abated. The time of such irregular intermissions varies from a few minutes to several hours, or even to several days. The patient then says that the pain comes on by spasms, and even medical men

are apt to hold the same language. This, however, is not a very correct application of the term spasm. Spasm means contraction, and the use of it ought to be restricted to involuntary contractions of the muscles. In applying it to nervous pains, as well as to muscular contractions, you confound together symptoms which, although they may arise from the same causes, are in themselves dissimilar, and you lead yourselves and others into error. Even where there is no absolute intermission, the intensity of the symptoms varies at different times, according to the state of the general health, the state of mind, and various other circumstances.

Nervous pains vary not only in degree but in kind. They are sometimes described as dull and wearying; at other times, and more frequently, as sharp, darting, or stabbing. A gentleman, who laboured under no other symptoms of disease, lost the sense of touch in one arm, and fore-arm and hand, so that the whole limb was benumbed, and, in the place of the natural sensations, experienced a sense of heat and burning, recurring at irregular intervals. Nervous pains may, in the first instance, be readily distinguished from those produced by inflammation by the absence of throbbing—by their not being increased by pressure—by there being no evident turgescence of the small vessels. But there is more difficulty in the diagnosis afterwards. As the commonest event may prove a source of annoyance to an irritable mind, so will nerves which have been kept for some time in a state of irritation transmit every impression that is made on them to the brain, with a disagreeable or painful sensation superadded to it: in other words, the part affected will be tender to the touch. And more than this: the tenderness may be followed by increased vascularity —by a slight degree of swelling—by actual inflammation. I do not mean to assert that any very active inflammation will be established—such as will end in suppuration and abscess, or ulcer; it will be moderate in degree, but it will be inflammation nevertheless, and marked by the usual symptoms. In a patient who had laboured for some time under pain in the testicle, depending on a calculus passing down the ureter into the bladder, the testicle became tender and considerably swollen. In a gentleman, who suffered for a great length of time from what was regarded as a most severe *tic douloureux* in the face, at first the parts to which the pain was referred retained their natural appearance, but ultimately they became swollen, from an effusion of serum into the cellular texture, and so exquisitely tender that they would not bear the slightest touch.

I have said that nervous pains are subject to irregular intermissions; but in some instances the intermissions are regular, and the returns of the pain are periodical, like those of an ague or intermitting fever. I have known such intermitting and periodical nervous pains to alternate with ague. In fact, the two diseases depend on the same state of the general system; and quinine, or arsenic, which would cure the intermitting fever, will also cure the intermitting pain. Here the character which the pain assumes

leads to an important rule of practice; but in other cases, as far as my own experience has yet gone, it teaches us but little as to the origin of the disease, or the remedies by which it is to be cured. What I am now about to mention renders it probable that the kind of pain depends (at the least) as much on the particular structure of the part to which it is referred, as on the particular cause which produces it. It has been stated, by Sir Henry Halford, that the tic douloureux in the face arises from the irritation of the nerves, occasioned by a portion of dead or carious bone, and I have no doubt that it is so in some instances. I have seen one, if not two cases, which confirm Sir Henry Halford's observation. But I also can entertain no doubt that it may arise from other causes. In one case, which I saw with Mr. Green and Mr. Freeman, the existence of epileptic fits, a ptosis of one eyelid, and some other symptoms, led us to believe that the pain in the face was the consequence of some disease in the brain. The patient died, and the appearances on dissection afforded ample proof of the correctness of the opinion which we had been led to form during the patient's life-time. In other instances it appears to be merely the consequence of a disordered condition of the digestive organs. But I am not aware that in these different cases there is any essential difference in the symptoms of the disease, or that it is possible for us, judging merely from the kind of pain, to pronounce that it arises from this or that cause, or that it is to be cured by this or that remedy.

Although there is no part of the body which may not, at one time or another, be the seat of these nervous affections, it would appear that some parts are more liable to them than others. They are met with less frequently in the viscera, which are supplied by the great sympathetic nerves, than in other parts. Nervous pains are more severe, and perhaps, on the whole, more common, in those parts which receive their nerves from the fifth pair—as the face, the eye, the tongue—than in any other individual part. Muscular spasms are common in the muscles of the neck, especially in the sterno-cleido-mastoideus. I am inclined to believe, also, that they occur more frequently in the upper limb than in the lower. It is not uncommon to see one hand and arm in a state of constant tremulous motion, there being no other indication of disease. I have seen several cases in which a muscular spasm of the upper limb has shown itself in the following manner. The patient experiences no inconvenience from it until he uses the limb; for example, until he sits down to write. Then, when he has gone so far as to have written a few letters, some of the muscles act involuntarily, and jerk the hand in a direction contrary to that which was intended; so that, instead of completing the word which was begun, the pen makes a long scratch on the paper.

A lady complained of pain in the head, and her mouth was drawn to one side; and hence she was supposed to suffer from paralysis of the muscles of one side of the face. However, when I was consulted respecting her, I observed that there were nearly

constant twitches of the cheek and eyelids on that side to which the mouth was drawn; and, on more minute examination, I was satisfied that the distortion of the mouth arose, not from the muscles on one side of the face being paralytic, but from those on the opposite side being in a state of spasm. The case precisely resembled that of a patient with spasmodic wry neck, except that the disease influenced a different set of muscles—namely, those supplied by the facial nerve, or the *portia dura* of the seventh pair.

Perhaps there are no muscles in the body which are, on the whole, more liable to have their actions deranged, under the influence of nervous disorders, than those of the pharynx and œsophagus. In not a few of those cases, which have been confounded together under the general appellation of stricture of the œsophagus, the disease is either a spasmodic or a partial paralytic affection of these parts; and the patient is to be cured, not by the introduction of bougies into the œsophagus, but by other means.

A lady consulted me concerning symptoms which were ascribed to a stricture of the œsophagus. She was unable to swallow the smallest morsel of solid food, so that she was compelled to subsist entirely on liquids, and even these she swallowed with great difficulty. These symptoms had been coming on for upwards of three years. I introduced a full-sized œsophagus bougie, which entered the stomach without meeting the slightest impediment. From this and other circumstances, I was led to conclude that the difficulty of deglutition was merely a symptom of some other disease. The lady's face was bleached, as if she had suffered from repeated attacks of hemorrhage, and her feet were in some degree œdematous. On enquiry I found that she had long laboured under internal piles, from which had taken place repeated discharges of blood. To this last disease, then, I directed my chief attention, prescribing a cold lavement to be injected every morning; and at the same time a solution of the sulphate of iron, and sulphate of quinine, to be taken three times daily by the mouth. When this plan had been persevered in for three weeks, the piles were much relieved; they no longer protruded externally; there had been no recurrence of hemorrhage; her cheeks were less pale, and she swallowed with comparative facility. At the end of six weeks more the piles occasioned very little inconvenience; she had lost no more blood; her general health was much improved; and there was so little difficulty of deglutition that I had no hesitation in recommending that, after her return to the country, she should swallow a bolus of Ward's paste three times daily, with a view to the cure of the hemorrhoidal disease.

The pathological history of these local nervous affections constitutes in itself a most curious and interesting object of research; but it has another and still stronger claim on your attention. Your patient wishes to be cured; he has, of course, no other reason for consulting you. Now you may supply yourselves with a list of what are called nervous remedies; prescribing, for example, the

carbonate of iron first, changing this for the extract of belladonna, and that for something else; trusting that accident will at last enable you to hit on the right expedient; but you will do little good by the adoption of such a loose and empirical method of practice. If you would cure your patient, you must study each individual case that comes before you, pathologically; endeavour to trace the symptoms to their true origin; and, if you can succeed in doing so, you will, in many instances, learn at the same time in what manner a cure is to be effected; while in others, in which the disease does not admit of a cure, you will learn this also: you will be enabled to avoid tormenting your patient with useless remedies; and at any rate you will be satisfied you can do as much for him as your neighbours.

It is not to be supposed that in these cases any permanent benefit can arise from applications made to the part to which the symptoms are referred, the cause on which they depend being elsewhere; and the first thing that you have to attend to in the treatment is, that you do not fall into the error of regarding the symptoms as constituting the original disease. A patient applies to you, complaining of a pain in the testicle; but the testicle appears to have its natural structure, and (except the pain) bears no marks of inflammation. You enquire further, and find that the pain is not constant; that it is especially induced by exercise, and that it subsides when the patient is in the horizontal posture. Examine the groin after he has taken a long walk, and you will perhaps find an incipient hernia; a small portion of bowel just attempting to protrude through the abdominal ring. You apply a truss, which supports the hernia, and cures the pain in the testicle. If you had been careless in your investigation of the case, and had applied leeches and lotions to the testicle, you would, to say the least, have plagued your patient to no purpose. Another person applies to you concerning a spasmodic wry neck. If you at once conclude that the disease is where it shows itself, and divide the tendon of the sterno-cleido-mastoideus muscle, what is the consequence? The patient undergoes a certain quantity of pain in the operation, and to no purpose; for, before the wound is completely cicatrised, the divided tendon has again become fixed by adhesion to the neighbouring textures, and the contraction of the muscle and the twisting of the neck are as bad as ever. I shall relate a case in which a patient underwent a severe and painful operation to no purpose, in consequence of such a want of discrimination on the part of the surgeon. A sailor had received a severe wound in the ham, I believe, from a musket ball. The wound healed, but not until after a considerable time, and the patient was left with a contracted leg, and suffering from a most agonising pain in the foot. This state of things having existed for a considerable time, and no benefit having been derived from any of the remedies employed, the poor fellow wished to lose the foot. The surgeon under whose care he was, therefore, amputated the leg; but, unfortunately, he amputated it, not above the knee and above the injury

of the nerve, but below the knee and below the injury. I scarcely need tell you the result. The pain continued as severe as ever, and it was not relieved until amputation had been performed a second time higher up in the limb.

It is, however, reasonable to conclude that few among you will be guilty of a mistake so palpable as this. But in many instances, as I have already expressed to you, the diagnosis is really difficult, and it will require a very minute observation, and much exercise of judgment, for you to understand the real nature of the case, so as to be enabled to determine where the primary disease is situated, and in what it consists. You must take into the account not only the present circumstances, but the former history; and your observations, instead of being limited to the particular symptoms concerning which you are consulted, must extend to the state of the animal functions generally; and where more light is wanted, you must be satisfied to wait and watch the further progress of the disease, and the effects produced on it by the remedies employed.

If the original disease operates immediately on the nerves of the affected part, producing in it pain, or muscular spasm or paralysis, you will have first to consider how far it is within the reach of topical remedies. If a tumour presses on a nerve—or if some foreign body, as a musket-ball, or a piece of dead bone, irritates its surface, or is entangled in its substance—perhaps the tumour or the foreign body may be removed by a surgical operation, or the tumour may be reduced by other means. If this cannot be accomplished, or if the nerve itself be altered in structure, either from disease or injury, it will become a matter for consideration, whether the limb should be amputated, or whether the nerve should be divided. It is only under these circumstances that any advantage can be expected to arise from the division of the nerve. In ordinary cases of neuralgia, where the disease on which it depends is in the brain or in some other distant part of the body, or where it is connected with some derangement of the general health, it is evident that such an operation cannot be recommended on any sound principle; and it need be a matter of no surprise, that, where it is performed, it should so generally fail. Where nothing better can be done, and a cure is not within your reach, a palliative treatment may be productive of some advantage; and you may endeavour to mitigate the patient's sufferings by the use of the local vapour bath, or by the application of the opium, or hemlock, or, what is still better, the belladonna plaster.

In other cases the success of your practice must mainly depend on these circumstances: whether you are able to discover the primary seat of the disease, and whether, if it be discovered, it is of such a nature as to be under the influence of remedies. If you refer to what I have said in former parts of the present lecture, you will find that I have anticipated much of what belongs to this part of our enquiries. I shall not trouble you by needless repetitions. There are some points, however, on which I feel it my duty to make some additional remarks.

The mucous membrane of the stomach and intestines presents a very extended surface, on which a multitude of nervous filaments are distributed, maintaining an extensive sympathy between these organs and the rest of the system. This membrane is subject to various causes of irritation, to which nervous affections, showing themselves even in distant parts of the body, may not unfrequently be traced. Hence it is that these diseases are in some instances relieved, or cured, by an adherence to a well-regulated diet, by the exhibition of purgatives, of what are called alterative medicines, and of others which tend to improve the disordered secretions of the stomach and liver.

In a great number of instances nervous pains are manifestly connected with a disposition to gout, and the colchicum, combined with other remedies, will contribute to their cure.

I have already adverted to cases in which various pains assume an intermitting and periodical character, having a manifest relation to cases of intermitting fever. According to my experience, there is no part of the body in which such pains may not occur; and when they occur daily, or on the alternate days, they are always relieved by the exhibition of the sulphate of quinine, or of the cinchona, combined with arsenic. But large doses of these medicines are sometimes required. A respectable medical practitioner consulted me, believing that he laboured under a disease of the spine. He complained of pain, which he referred to the inferior dorsal vertebræ, and which was so severe that he could, as he said, scarcely endure it. On enquiry, I learned that the pain attacked him always at a particular period of the night; that it lasted for a certain number of hours, and that he was free from pain, or nearly so, in the intervals. I recommended that he should take the sulphate of quinine procured at Apothecaries' Hall. He took as much as fifteen or sixteen grains daily without any decided amendment; but I was so satisfied of the efficacy of the remedy in such a case, that I advised him to increase the dose still further. At last he took half a dram of the sulphate of quinine daily, and this effected his cure.

Nervous affections of the same kind not unfrequently show themselves in other ways; still they are cured by the same remedies. It would be an endless task for me to describe all the varieties of this disease which you will meet with in practice; and I shall content myself with furnishing the following examples.

In my lecture on the diseases of the urinary organs, I have noticed the case of a gentleman who had long laboured under a stricture of the urethra; but from which, introducing a bougie occasionally for himself, he suffered little. At last he became affected with a periodical retention of the urine, recurring at a certain hour every night. The retention continued for some hours, and then subsided. The introduction of the catheter gave him relief at the time by emptying the bladder, but it did not remove the spasm; and, if the urine was secreted rapidly afterwards, a second introduction of it was required. After this state of things

had continued for some time, I prescribed for him two grains of the sulphate of quinine to be taken every six hours. On the first night after he began to take it the retention recurred, but he had no attack afterwards.

A lady, about sixty years of age, complained of a most distressing sensation of thirst, beginning about ten o'clock in the forenoon, continuing for five hours, and recurring daily. A slight degree of chilliness preceded the attack; and while it lasted, although the sense of thirst was such as to produce absolute misery, there was no perceptible dryness of the mouth and fauces, and the secretion of urine was natural. These symptoms had existed for several weeks. The patient appeared to labour under no other disease; she had, however, begun to lose flesh, and her complexion was sallow. The same symptoms had attacked her four years ago. At that time they continued for six months, leaving her thin and debilitated. I prescribed for her three grains of the sulphate of quinine, to be taken three times daily. I have not seen her since; but at the end of four days I received a note to the following effect: "Mrs. ———, the thirsty lady, has the pleasure to say that she is very much better, and she is much obliged to Mr. Brodie for his advice. She returns to the country to-morrow."

A lady suffered from a neuralgic affection of the face. Her medical attendant prescribed a preparation of valerian, and the pain in the face subsided; but immediately afterwards she began to experience a pain in one foot. This pain recurred in the early part of every evening. After a short time it was followed by redness of the skin, and tumefaction of the subjacent parts near the bases of the toes. These marks of inflammation continued to increase for some hours, and then subsided, leaving the foot of its natural appearance and free from pain. This state of things, at the time of my being consulted, had existed with little variation for several months. She was advised to take the sulphate of quinine. On the following evening the attack was less severe than formerly, and in the course of three or four days the symptoms had entirely subsided.

In this case the inflammation of the foot was manifestly the consequence of the intermitting neuralgia. In that which follows, the inflammation of the leg formed the most prominent feature of the disease; yet, from its resemblance to the last, we can scarcely doubt that it ought to be considered as belonging to the class of nervous affections.

A lady laboured under an inflammation of her leg. The whole leg was swollen from the toes to the knee—the skin being red, painful, and tender. These symptoms had existed for several weeks; the usual remedies had been employed, and no amendment had taken place; yet the inflammation did not proceed further, and there were no signs of suppuration. At last I observed that the symptoms varied considerably; that sometimes the redness, pain, and swelling had nearly subsided—that at other times they were as strongly marked as ever; and that these variations always

took place on the alternate days. She was now directed to take the sulphate of quinine. The effect was immediate, and a few days completed the cure.

In those cases in which the local nervous affection depends on an organic disease of the brain, or spinal marrow, it is evident that the patient has no chance of actual cure. Other nervous symptoms show themselves in succession; such as a stumbling walk, a drawling speech, epileptic fits, derangement of the intellect, and at last a stroke of apoplexy occurs as the immediate prelude of death. But here months or years may elapse before the disease reaches its fatal termination; and in the mean time you attain an important end, if you can relieve the local symptoms. Now, where these appear in the form of muscular spasms or paralysis, according to my experience, remedies are of little avail. The spasms may subside spontaneously, but they are not to be relieved by art. It is different, however, with respect to nervous pains; and for these, local applications of hemlock or belladonna, stimulating liniments combined with laudanum, and even blisters, may be employed with advantage—removing the pain, perhaps for a time, perhaps permanently, although the disease on which the pain depends is slowly but progressively advancing.

Another very extensive class of local nervous affections remains to be investigated. To these I shall call your attention in the next lecture.

LECTURE II.

Various forms of local hysterical affections.

When I was formerly engaged in the study of the diseases of the joints—having, at the period to which I refer, few opportunities of investigating the subject except in my hospital practice—I occasionally met with cases in which a particular joint was affected with pain, and a great degree of morbid sensibility, attended occasionally with some degree of tumefaction of the soft parts, although the characteristic symptoms of the ordinary diseases to which these organs are liable were wanting, and the usual consequences of abscess and destruction of the joint did not ensue. For a long time these cases occasioned me great perplexity; and it was not until after I had published the first edition of my Treatise on the Diseases of the Joints, that the occurrence of the case which I am about to describe first led me to suspect the real origin of the symptoms, which I had not comprehended formerly.

I was consulted concerning a young lady who complained of severe pain and a morbid tenderness of the knee, in the first instance attended with no perceptible enlargement of the joint. The

remedies which, with such knowledge as I then possessed, I was led to recommend, gave her no relief; and after some time a slight degree of tumefaction took place, depending, as it seemed, either on a fulness of the small vessels, or on an effusion of lymph or serum into the subcutaneous cellular texture. She had been in this state for a considerable time, when she was seized with a succession of violent paroxysms of hysteria, which terminated in an hysterical affection of the brain; so that she lay in a state approaching to that of coma, with dilatation of the pupils of the eyes. She was now attended by the late Dr. Babington and myself. I do not undertake to say whether the disease yielded to the remedies employed, or reached its natural termination; but, from one or other of these causes, the patient recovered of the last-mentioned symptoms, and from that time she never complained of her knee.

Not long afterwards, another young lady was brought to me, labouring under what had been supposed to be a scrofulous disease of the wrist. The resemblance of this case to that of the last-mentioned patient led me to doubt the correctness of this opinion, and the results proved my doubts to be not without foundation. She also was seized with a succession of violent paroxysms of hysteria; and when, after the lapse of many days, she had recovered from them, the disease of the wrist had vanished.

It seemed impossible to doubt that in each of these cases there was some connection between the local symptoms and the constitutional disease under which the patient laboured; and a great number of other cases, which fell under my observation afterwards, confirmed me in the opinion—that where there is that state of the general system, whatever it may be, which produces the phenomena of hysteria, it is not uncommon for a particular joint to be affected with pain and morbid sensibility, such as may lead a superficial observer to believe that it is the seat of some serious local disease, although no such disease in reality exists.

In the second and subsequent editions of my Treatise on the Diseases of Joints, I have given some account of these local hysterical affections. I trust that what I have there stated may have been not wholly unacceptable to those who are engaged in the practice of our art; but the subject is one of great interest both to the scientific pathologist and to the practical surgeon; and, believing that I can furnish you with some information respecting it, beyond that which is to be found in these publications, I am led to call your attention to it on the present occasion.

I have already mentioned, that when my opportunities of studying these diseases were limited to what I saw in the wards of the hospital, these affections of the joints fell occasionally under my observation. Since I have been engaged in a large private practice, they have presented themselves, I may say without exaggeration, almost daily. This is easily explained, "*Fœminarum enim paucissimæ*," says the sagacious and observing Sydenham, speaking of hysteria, "*ab omni horum adfectuum specie prorsus liberæ sunt, si istas excipias quæ laboribus adsuetæ duram vitam tra-*

hunt." The liability to hysteria is, in fact, among females, one of the severest penalties of high civilisation. It is among those who enjoy what are supposed to be the advantages of affluence and an easy life that we are to look for cases of this description—not among those who, fulfilling the edict of the Deity, "eat their bread in the sweat of their face." I do not hesitate to declare that, among the higher classes of society, at least four fifths of the female patients, who are commonly supposed to labour under diseases of the joints, labour under hysteria, and nothing else.

Frequently the symptoms are referred to the hip joint. They then have a considerable resemblance to those of diseases in the bones or cartilages, yet a minute examination of the case will rarely leave you in doubt as to your diagnosis.

There is pain in the hip and knee, which is aggravated by pressure and the motion of the limb, and the patient often lies fixed in one position on the bed or sofa. You will say, "are not these indications of a diseased hip joint?" But observe further. The pain is not in general fixed in any one part—it belongs to the whole limb. The patient winces, and sometimes screams, when you make pressure on the hip; but she does the same if you make pressure on the ileum, or on the side as high as the false ribs, or on the thigh, or even on the leg as low as the ankle; and every where the morbid sensibility is chiefly in the integuments. If you pinch the skin, lifting it at the same time off the subjacent parts, the patient complains more than when you forcibly squeeze the head of the thigh-bone into the socket of the acetabulum. As her attention is more directed to the examination, so the pain which she suffers from it is aggravated; and, if her mind be occupied in conversation, she will scarcely complain of that which would have occasioned torture otherwise. Then, there is no wasting of the *glutæi* muscles, and no flattened appearance of the nates; and the aspect of the patient is different from that which you would expect to find if the bones and cartilages of a joint were in a state of ulceration. Neither are there those peculiar and painful startings of the limb at night, attended often with frightful dreams, which mark the existence of this last disease. The pain will sometimes prevent the patient falling asleep, but, if once asleep, she sleeps soundly for many successive hours; and this state of things may continue for weeks, or months, or even for years, without leading to abscess or any further ill consequences. There may be a suspicion of abscess (I have known this in a great number of instances), but the suspicion is never realised. Sometimes there is a general tumefaction of the thigh and nates—the consequence either of a turgid state of the small vessels, or of an effusion into the cellular texture (I suppose of the former, as the parts do not *pit* or remain indented after pressure); but this is entirely different from the swelling of an abscess. In a few rare instances there is a more defined and circumscribed swelling, but still it is altogether different from that of abscess. There is no perceptible fluctuation, and I can compare it to nothing better than a wheal of urticaria of

unusual magnitude. A careful examination will always enable you to distinguish these swellings from abscess. For the satisfaction of others, I have sometimes made a puncture with a grooved needle, or some other convenient instrument, the introduction of which would have detected matter, if matter had existed.

I have said that in these cases there is no wasting of the glutæi muscles, and no flattened appearance of the nates. It is, however, not uncommon to find much alteration in the figure of the parts, of another kind; namely a bulging of the pelvis posteriorly, at the same time that it is elevated, on the side of the disease, so as to make an acute instead of a right angle with the column of the vertebræ. Of course, under these circumstances, the limb is apparently shortened; and, when the patient stands erect, the heel does not come in contact with the ground. A superficial observer may be led to believe that there is an actual dislocation of the hip; and, indeed, it requires a careful examination to enable the surgeon to understand that all this strange distortion is but the result of the predominant action of certain muscles, and of a long-continued indulgence in an unnatural position.

When the symptoms are referred to the knee, they bear a near resemblance to those which have been just described. There is great tenderness of the joint; but the patient suffers more from pinching the skin than from pressure, and the morbid sensibility extends for some distance up the thigh, and down the leg, perhaps as low as the foot and ankle. She suffers less from an examination when the attention is fixed on other matters, than when it is directed to the affected parts; and she does not usually complain when pressure is made on the heel, so as to press the articulating surface of the tibia against that of the femur, provided that care be taken at the same time to produce no motion of the joint. In most instances the leg is kept extended on the thigh, whereas, in cases of real disease in the knee joint, it is usually a little bent. The symptoms may continue in this case, also, without any material alteration, for an indefinite time—for weeks, or months, even for years, the joint retaining its natural size and figure; but occasionally a slight degree of tumefaction is observable, especially on the anterior part, over and on each side of the ligament of the patella. This tumefaction is not to be confounded with a general enlargement of the joint, by which surgeons are frequently perplexed and misled—the result not of the disease, but of the remedies employed. I refer to cases which have been misunderstood, and mismanaged by the application of blisters, issues, and a succession of various counter-irritants.

What I have now stated may be sufficient to enable you to understand the nature of the symptoms which you may expect to find, where these hysterical affections occur in the other joints of the extremities. The following observations are equally applicable to all these cases, and, while they are necessary to complete the history, will be found of use in enabling you to form a correct diagnosis.

The patients thus affected are, for the most part, not much above the age of puberty.

In many instances they labour under some irregularity with respect to menstruation; while in others this function is in no respect different from what it is under circumstances of perfect health.

Those who labour under habitual coldness of the hands, have a weak small pulse, and afford other indications of a feeble circulation, are more liable than others to suffer in this manner; yet occasionally we find these symptoms existing in combination with a florid countenance and a sufficient development of animal heat.

In some instances the joint to which the symptoms are referred, and even the whole limb, is affected with a remarkable alternation of heat and cold. Thus, in the morning the limb may be cold, and of a pale or purple colour, as if there were scarcely any circulation of blood in it; while towards the afternoon it becomes warm, and in the evening is actually hot to the touch, with the vessels turgid and the skin shining. This state of things is often a source of serious alarm to the patient, and even to the medical attendant, but I never knew it to be followed by any ill consequences.

The majority of the patients thus affected exhibit other proofs of their liability to hysteria. Sometimes they have been subject to the usual paroxysms of hysteria, which have ceased on the local symptoms showing themselves; and a recurrence of the former has been followed by an abatement of the latter, or by complete recovery from them.

Not unfrequently the origin of these symptoms may be traced to a severe illness, which has left the patient in a state of great physical exhaustion; at other times they are as clearly to be attributed to some moral cause having a depressing influence on the constitution. In like manner the agency of moral causes, especially of those which compel the patient to make much physical exertion, often leads to her recovery. But we must not be led by this last-mentioned circumstance to adopt the harsh conclusion, that these symptoms exist only in those who are of a fanciful and wayward disposition. Young women of the highest moral qualities, and of the strongest understanding, are not exempt from these maladies; but it must at the same time be acknowledged that a cure is more easily attained in them than it is in others.

Although there are none of those painful and involuntary startings of the limbs which occur in combination with caries of the joints, spasmodic actions of the muscles of the limbs are not uncommon in the cases of which I am now speaking. In some instances convulsive motions of the limbs are produced by pinching, or even by lightly touching, the integuments. These bear no very distant resemblance to the movements of chorea; and it is worthy of notice, that they do not occur if it can be managed at the same time that the attention of the patient should be otherwise directed. I have also known them to take place independently of any manifest exciting cause. In some cases which have fallen

under my observation, the limb was at irregular periods violently agitated, so as almost to throw the patient off her couch.

In these cases there is always a sense of weakness in the limb, which, for obvious reasons, becomes aggravated in proportion as the muscles have been for a longer time in a state of inaction. While the pain and morbid sensibility of the joint are gradually subsiding, this sense of weakness increases, until at last it is the predominant symptom. Under these circumstances the patient often says, "I have no pain, but I cannot walk, because the limb is so weak." Weakness of the muscles, however, is not the only circumstance which interferes with the speedy recovery of the use of the limb in these cases. The tunics of the small blood-vessels, when the limb has been long kept in the horizontal posture, seem to partake of the condition of the muscles; and when the foot is first put to the ground, the skin assumes, in consequence, a red colour, sometimes amounting to a purple hue, as dark as that which, when limited to a particular spot, is often the precursor of a vesication.

The symptoms which have been described for the most part come on gradually. In the majority of cases they subside gradually also; but sometimes it is otherwise, and they vanish all at once without any evident cause. For example: in the year 1834, I was consulted respecting a young lady labouring under a well-marked hysterical affection, simulating disease of the hip joint. As she was not a resident in London, I had no opportunity of watching the progress of the case, but I have lately received the following account of it from Dr. Mortimer, the surgeon of Haslar Hospital:—Her symptoms had continued, nearly unaltered, for almost two years, when one night, on turning herself in bed, she said that she had a feeling as if something had given way in her hip, and from that moment she was quite well.

Another young lady was brought to London for my opinion in October, 1833. She also was supposed to labour under a disease of the hip joint. After a careful examination of her case, I was satisfied that it was one of hysterical affection, and that there was no actual disease of the joint. I recommended her to leave her couch, to which she had been confined, and to take exercise, especially on horseback. Being a sensible and well-disposed person, she followed this advice, in spite, I doubt not, of a good deal of inconvenience in the first instance. After the lapse of a year, I received from her father the following statement respecting her:—"In pursuance of your advice, she began to use the limb more freely, but with little alteration as to pain and lameness until about six weeks ago, when, by a fall of the donkey on which she was riding, she was thrown over the animal's head, standing on the foot of the lame limb, with her weight upon it. She felt immediately what she describes as a sudden snap, as if something near the joint had given way. This was attended with a violent acute pain, which, however, lasted only a short time. She was replaced on the donkey, and rode home, a distance of four miles. To her

great surprise, the former habitual pain of the limb had entirely discontinued, and there has been no return of it since. She was able to walk up and down stairs without difficulty or pain, and now walks a considerable distance, using the one leg as freely and as well as the other. Her general health is improving rapidly, although she is still weak. There has been no hysterical fit since the accident; in short, the cure has been complete." However, the cure was not permanent. Three months afterwards the complaint recurred, having the same character as formerly, except that it was not now combined, as it had been in the previous attack, with other hysterical symptoms. She was at this time on the continent, and I have not heard the result of the case.

I have hitherto described these cases as if the symptoms were peculiar to the female sex, but it is not so in reality; and I have known several (though by comparison certainly rare) instances of males being affected in the same manner. I employ the term hysteria because it is in common use, and because it would be inconvenient to change it for another; but the etymology of it is undoubtedly calculated to lead to a great misapprehension with respect to the pathology of that disease. It belongs not to the uterus, but to the nervous system; and there is no one who is much engaged either in medical or in surgical practice, who will not be able to bear testimony to the accuracy of Sydenham's observation on this subject:—"*Quinimmo non pauci ex iis viris qui vitam degentes solitariam, chartis solent impallescere eodem morbo tentantur.*"

Hysterical affections, in which the symptoms are referred to the spine, are of very frequent occurrence. Such cases are, in many instances, mistaken for those of ulceration of the intervertebral cartilages and bodies of the vertebræ; and in consequence of this unfortunate impression on the minds of the medical attendants, I have known, not a few but very numerous, instances of young ladies being condemned to the horizontal posture, and even to the torture of caustic issues and setons, for several successive years, in whom air and exercise, and cheerful occupations, would probably have produced a cure in the course of a few months.

In these cases the patient complains of pain and tenderness of the back, to which one or more of the following symptoms may be superadded, tending very much to mislead the medical or surgical attendant: pains in the limbs, especially in the lower limbs; a sense of constriction of the chest; involuntary spasms of the muscles, sometimes induced by change of position, at other times recurring without any very evident cause; a sense of weakness in the lower limbs, so that they are scarcely capable of supporting the weight of the body; and even actual paralysis; difficulty of voiding the urine. When the patient first complains of pain in the back, it must be acknowledged that there is some difficulty in forming a positive diagnosis; but the difficulty vanishes afterwards, so that none but a superficial observer can have any doubt as to the real nature of the disease. The pain in the back is seldom

confined to a single spot, but it extends to different regions of the spine, and it not unfrequently shifts its place from one part to another. The tenderness of the spine is peculiar. The morbid sensibility is chiefly in the skin, and the patient, for the most part, flinches more when the skin is even slightly pinched than when pressure is made on the vertebræ themselves. The pain is, in the majority of cases, more severe than in those of real vertebral disease; and the spasms of the muscles have a near resemblance to those of chorea. Where there is paralysis, or a tendency to paralysis, it is quite different from what is observed in cases of pressure on the spinal cord or brain; and I may take this opportunity of observing, with respect to hysterical paralysis generally, that it has this peculiarity: *it is not that the muscles are incapable of obeying the act of volition, but that the function of volition is not exercised.* The accuracy of this observation will, if I am not much mistaken, be acknowledged by all those who are at the pains of studying these cases with the attention which they so well deserve; and the importance of it in medical and surgical practice is sufficiently obvious. There are still other circumstances which may assist us in forming our judgment: such as the general aspect of the patient, and her condition in other respects; her time of life; the state of the menstruation; and, especially, the liability to the more common hysterical affection.

Patients with a weak pulse, and cold hands and feet, are, on the whole, more liable to suffer in this manner than other persons. But this is almost a needless repetition. It would be sufficient for me to refer to what I have already stated, in speaking of hysterical affections simulating diseases of the joints of the extremities.

I have frequently known surgeons to apply a hot sponge to the spine, believing that if the patient complained of pain on the application, this was a proof of the existence of caries. My own experience leads me to believe that a patient who labours under a nervous pain of the back will complain of the hot sponge even more than one in whom real disease exists. I mention this trifling matter, that you may avoid being misled by it in your diagnosis.

What I have already described are only a part of the local hysterical affections which fall under the observation of the surgeon, and an acquaintance with which is necessary, to enable him to practise his art with credit to himself and advantage to the public.

Hysterical retention of urine is of such frequent occurrence, that any particular description of it would seem to be superfluous. An observation, which has been already made, is equally applicable to this as to other forms of hysterical paralysis. The muscles are not incapable of obeying the act of volition, but the volition itself is not exercised. So it is, at least, in the first instance: but if the patient has allowed the bladder to remain for a great length of time in a state of extreme distention, actual paralysis may ensue, and she may then strive in vain to empty the bladder without the aid of the catheter. In these, and in other cases in which the

bladder has been long extremely distended, the mucous membrane becomes affected with chronic inflammation, secreting the usual adhesive mucus; and even worse consequences may ensue than these. In a case to which I have had occasion to refer in my lectures on the diseases of the urinary organs, hysterical retention of urine having been for a long time neglected, at last forty ounces of urine were drawn off by the catheter. In the *post-mortem* examination, the bladder was found of a very large size, of a dark and almost black colour: there were only slight vestiges of its natural structure left, the muscular fibres being very much wasted, and the internal membrane presenting the appearance of a very thin film, which was readily separated from the parts below. The dark colour of the bladder did not seem to arise from mortification, there being no fetor, nor, with the exception of the black colour, any indication of it.

Females who labour under hysterical retention of urine, if left to themselves usually recover in the course of a short space of time —sometimes almost suddenly; but if the catheter be employed, their recovery may be protracted for an indefinite period. We may lay it down as a general rule, that in these cases the catheter should not be had recourse to: and the only exceptions to it are in those extreme cases in which actual paralysis has taken place, and the bladder is likely to become diseased, if not artificially relieved.

Hysterical *aphonia*, or loss of voice, allowance being made for the different functions of the affected parts, corresponds very nearly to the hysterical retention of urine. It takes place suddenly, continues often for many months, even for one or two years; and then disappears as suddenly as it began. A patient thus affected may, when under the influence of strong moral excitement, find herself speaking in her natural voice, when, for some time before, she had spoken only in a whisper. Her recovery may be permanent, or she may relapse into her former condition. This symptom is not unfrequently met with in the male sex, especially in those of the clerical profession, probably because they often lead very sedentary lives, and also because in their profession they are called upon to speak in public in a tone raised above the ordinary standard.

A tympanitic distension of the intestines is not an uncommon symptom in young women who are affected with hysteria; and, when existing to a great extent, is frequently mistaken for ovarian dropsy. The majority of cases in which the patient has been supposed to be cured of ovarian dropsy, by the agency of iodine and other remedies, have been, I doubt not, of this description. Yet the diagnosis is not difficult. The absence of fluid is distinguished by the absence of fluctuation; and the sound produced by percussion sufficiently indicates the cause of the distension. When the tumour is of a large size, there is a pain in the abdomen, and the respiration is rendered difficult in consequence of the impediment which exists to the descent of the diaphragm. If the uneasiness be such as to induce the practitioner to direct the use of the warm

bath, and the tympanitic distension be great, the effect is remarkable. Instead of sinking in the bath, as under ordinary circumstances, the patient floats in the water. If an elastic gum tube be cautiously introduced, so as to reach the upper part of the rectum, and pressure be made on the surface of the abdomen, the air may, in some instances, be made to escape through the tube, until the abdomen is reduced almost to its natural dimensions; but it becomes re-accumulated in the course of a few hours. A stimulating injection, made with the *confectio rutæ*, will sometimes produce the same result.

Young women are subject to an affection of the breast, corresponding to the hysterical affections of the joints, and indicated by very similar symptoms. These cases have been noticed by Sir A. Cooper, in his observations on the diseases of the breast. The patient complains of pain in the breast, and shrinks on pressure being made with the fingers, or even on the skin being slightly pinched. Not unfrequently the examination of the part produces twitches and motions of the body, bearing no small resemblance to those of chorea; yet, if it can be dexterously managed, while the examination is being made, that the patient's attention should be otherwise engaged, not only these motions do not occur, but she may seem scarcely sensible of pain. The morbid sensibility is not confined to the breast, but extends to the axilla, and down the arm. No distinct tumour is perceptible in the breast; but when the disease has been of long continuance the whole organ becomes slightly enlarged, probably in consequence of an increased determination of blood to the small vessels; yet there is no redness of the skin, and indeed the skin is even paler than natural, with a somewhat glossy appearance of its surface.

These cases are to be distinguished from those of a rare kind of irritable tumour of the breast, of which a representation is to be found among the plates annexed to Sir Astley Cooper's work. I conceive that they ought also to be distinguished from those which may occur at any time of life, and in women who have no particular disposition to hysteria. In the cases to which I now allude, the pain and tenderness are much less than in the true hysterical affection of the breast, and it will be almost invariably found that the patient has witnessed the miseries of some friend or acquaintance who has suffered from carcinoma. No part of the body will bear that rigid scrutiny to which the breast is subjected under these circumstances. Close attention will discover in any, even in the most healthy organ, sensations which have been previously overlooked; and constant anxiety on the subject may magnify such sensations into pain. In these last mentioned cases a strong assurance that no disease exists will make the patient happy, and remove the pain; but no assurance will be adequate to the cure of a genuine hysterical affection.

Hysterical tympanitis is always attended with a more or less constipated state of the bowels. But obstinate constipation of the bowels is a frequent occurrence in hysterical patients, independently

of any considerable degree of tympanitis; and I have known many instances in which a case of this kind has been mistaken for one of stricture in the upper part of the rectum. The surgeon here sometimes misleads himself by taking it for granted that a very long bougie may be introduced into the rectum, if there be no actual contraction; not recollecting that the naturally tortuous course of the bowel is often sufficient to prevent a bougie being passed more than a few inches, even in a healthy rectum. But the statement of the patient tends to mislead him also; for she describes herself as going to the water-closet, and yet being unable to eject the contents of the bowels. I will not say that it is so in all cases, but I am satisfied that, in some instances, if you cross-examine the patient, you will find reason to believe that the hysterical constipation of the bowels is of the same nature with the hysterical retention of urine. The effort of volition is not exercised except when the accumulation of fæces has become excessive. Hysterical difficulty of deglutition, which is sometimes mistaken for stricture of the œsophagus, is probably an affection of the same kind; there being no actual spasm, but a defective action of the voluntary muscles, by means of which deglutition is performed.

Symptoms resembling those of tetanus occasionally occur in patients who are under the influence of hysteria; sometimes assuming the form of trismus, at other times that of *opisthotonos*. A case of *locked jaw*, cured by the injection of oil of turpentine into the rectum, and published by Dr. Philips (then residing at Andover), in the sixth volume of the Medico-Chirurgical Transactions, is manifestly one of this description.

In a great number of instances, local hysterical symptoms appear to be connected with some accidental injury; generally a very slight one; and they are then especially liable to be misunderstood, and mistaken for something very different from what they really are.

For example: a woman is bled in the arm. She complains, perhaps, of severe pain at the time; but this subsides, and the wound heals, as under ordinary circumstances. Then she complains of pain again, extending down the fore-arm to the hand, up the arm to the axilla and shoulder, and even to the side of the neck, and sometimes down the side of the chest also: the extent and degree of pain varying in different cases. You examine the cicatrix, but can discover nothing unusual in it; but the patient flinches when it is touched. She very commonly complains of the surgeon, saying that she was badly bled, or bled with a blunt lancet, or a foul lancet, or that a nerve was pricked which ought not to have been touched; while the real origin of her symptoms may be traced to the peculiar state of her own nervous system. If you investigate the case further, you will always find that she has been liable to various nervous symptoms previously to those which are attributed to her being bled; and when these last disappear, nervous symptoms of some other kind show themselves.

In another case, the patient has received a blow on the head.

In order to avert the consequences which such an injury may be expected to produce, she is bled repeatedly, takes aperient medicines, and is kept on a low diet. When her physical powers are thus reduced, she complains of pain in the head even more than she did in the first instance: but the pain is of a different character, and is usually attended with other symptoms, such as do not belong to inflammation. Thus she has a sense of dizziness, or a feeling as if water was trickling over her head. Then the countenance is blanched, the skin is cool, and the pulse is probably small and quick, and weak. If, under these circumstances, the surgeon, mistaking the nature of the case, continues to abstract blood, and to keep the patient on a low diet, all these symptoms become aggravated; other symptoms of a more decidedly hysterical character show themselves, and no improvement takes place until a more judicious treatment is adopted. In another case, which is of no unfrequent occurrence, a young woman pricks her finger, or perhaps the finger is merely pinched. Soon afterwards, she complains of pain extending from the finger upwards, along the hand and fore-arm. This probably is followed by a convulsive action of the muscles of the arm, or by a continued contraction of the flexor muscles on the anterior part of the arm, so that the fore-arm is kept permanently bent; at least while the patient is awake, for the spasm is generally relaxed during sleep.

But the symptoms which, in hysterical patients, are attributed to a local injury, often proceed much further than what I have hitherto described. For example:—

A young lady, eleven or twelve years of age, pricked the fore-finger of her left hand with the point of a pair of scissors. This was immediately followed by pain in the course of the median nerve, and on the following day the fore-arm was fixed, by muscular contraction, at a right angle with the arm. After a few days, all the muscles of the hand and fore-arm were affected with violent spasms, producing strange convulsive movements of the hand and fore-arm. These were attended with sickness and vomiting, so that, for two days, whatever was received into the stomach was immediately rejected from it. By degrees the other limbs became affected in the same manner, and it was impossible for the patient to walk, or even to stand. Sometimes the diaphragm was affected so as almost to threaten suffocation. At other times the jaw was closed by a contraction of the masseter muscle, or she lay in a state of opisthotonos. Occasionally there was a violent pain in the head, which was described as having the same character as that of the finger which had been pricked; and these symptoms continued (sometimes one order of them, sometimes another, being predominant) until recovery took place under the circumstances which I shall have occasion to notice hereafter.

With a view to the further illustration of this part of the subject, I shall mention another case. A female, about thirty years of age, was admitted into St. George's Hospital, on account of a simple fracture of both bones of the fore-arm. There was nothing unusual in

the fracture, but she complained of an extreme degree of pain in the injured part. By degrees the pain extended up the arm to the axilla; then to the same side of the neck and head. The smallest motion of the limb, even the lifting the fore-arm off the pillow on which it lay, occasioned violent pain, and convulsive agitation of the limb, which were soon followed by what might be termed a state of hysterical syncope, in which the patient lay apparently insensible to external impressions for several minutes. The fracture united as under ordinary circumstances; but the nervous symptoms continued for many weeks, then subsiding gradually. It is worthy of notice (and this circumstance confirms the opinion that symptoms of this kind belong more to the constitution than to the actual injury) that, about two years before the occurrence of this last accident, this individual had met with a slight injury of the ankle, for which she was attended by Mr. Fuller, of Piccadilly; and that a train of nervous symptoms at that time supervened, nearly similar to those with which she was afterwards affected in the hospital. It is also worthy of notice, that on both occasions she had occasionally a spitting of blood, probably furnished by the mucous membrane of the pharynx or trachea, as there was no reason, either at the time or afterwards, to suspect the existence of disease in the lungs.

I have seen several cases of a singular affection of the hand and wrist, which manifestly belong to the class of cases of which we are now treating. It occurs in females who have a disposition to hysteria, especially in those who have suffered from mental anxiety and over-exertion, and is usually, but not constantly, referred to a sprain, or some other slight accident. The patient complains of pain in the back of the hand and wrist, trifling at first, but gradually becoming more severe. In many instances, after some time has elapsed, there is a diffused swelling of the soft parts, extending a short distance up the lower extremity of the fore-arm, and downwards as low as the fingers. This swelling is not attended with redness of the skin; and having lasted for a few weeks, it subsides, while the pain remains, constant in its character, aggravated by every motion of the limb, and always more severe in proportion as the patient's attention is in a greater degree directed to it. To prevent the motion, which she so much dreads, the patient keeps her hand in one position, and the consequence is that the joints become comparatively stiff, the hand at the same time having a very characteristic appearance, the skin being smooth and shining, and appearing to adhere more closely than is usual to the parts beneath. This state of things may continue for three months, for six months, or even for one or two years; the symptoms then gradually subsiding, without leading to any further ill consequences. The result, however, is not always so fortunate. I attended a lady who laboured under the symptoms which I have just described, with the late Dr. Luke. She left London on a visit to the continent, without any amendment having taken place. I saw her again after the lapse of four or five years: the muscles of

the fore-arm were this time wasted and paralytic; the whole hand was shriveled and useless; the fingers permanently contracted towards the palm of the hand; the nails thin and scabrous.

I shall conclude the present lecture by a brief notice of some cases which will serve to illustrate further the variety of singular local symptoms which may arise as a consequence of hysteria, and which may fall under your observation as practitioners in surgery.

I was consulted concerning a young lady, eighteen years of age, under the following circumstances. She was liable to fits of incessant sneezing, attended with a most abundant flow of watery fluid from the nostrils. This sometimes alternated with a nervous cough; while at other times she suffered from that sensation in the throat which is usually described under the name of *globus hystericus*. Not unfrequently she was affected with ordinary paroxysms of hysteria. She had a feeble circulation, and cold hands and feet, and her menstruation was irregular and deficient; in other respects she was in good health. There was no evident disease in the nostrils.

A married lady, thirty-seven years of age, was affected with similar fits of sneezing, attended also with a copious watery discharge from the nostrils. These symptoms attacked her once in a week, and in each of these attacks she sneezed not less than one hundred times; the watery fluid dropping from the nostrils so as to wet a pocket-handkerchief completely through. About the same time she began to experience a disagreeable sensation in the face and palate, not amounting to pain, but which she described to be such as might be produced by a worm creeping in her flesh. These latter symptoms gradually became more distressing, while the fits of sneezing became less frequent. At the time of my being consulted, three years after the commencement of the disease, the fits of sneezing do not occur oftener than once in a month, but she complained of an aching pain, with a sense of pulsation, in the roof of the mouth, the teeth, and tongue, occurring chiefly during the night, and being then very severe. There were no perceptible marks either of inflammation, or of other disease, in the parts to which the pain was referred.

An unmarried lady, thirty-two years of age, consulted me on account of her being liable to some very distressing paroxysms, in which she experienced a difficulty of respiration, attended with a sense of constriction of the chest, and great general excitement and agitation. These paroxysms often continued for ten or fifteen minutes, recurring at irregular intervals; sometimes without any evident cause; while at other times they might be traced to some sudden emotion of the mind. So far the case did not differ from many other cases of hysteria; but the peculiarity of it, and the circumstance which led to my being consulted, was as follows:— There was a particular spot near the ensiform cartilage, which she believed to be in some way or another connected with her complaint. Nothing could be discovered in this part different from what is usual, by the most strict examination; but the pressure of

the finger on it never failed to induce one of the paroxysms which I have just described. When these paroxysms were most severe, they were always attended with an abundant flow of limpid urine. These symptoms had existed in a greater or less degree for ten or twelve years, and had supervened on a state of exhaustion, occasioned by an attack of typhus fever.

A young married lady, who was liable to ordinary attacks of hysteria, complained of a tender spot on the anterior part of the abdomen, a little below the ensiform cartilage. The slightest pressure of the finger on it caused excessive pain, and was followed by violent agitation of the whole person, bearing a more near resemblance to the convulsive motions of *chorea* than to any thing else, and continuing for several minutes.

LECTURE III.

Pathology of hysteria—Treatment of local hysterical affections.

Although the examples of local hysterical affections which I have adduced in the two preceding lectures form only a part of those which you will meet with in practice, they are probably sufficient to answer the purpose of rendering you less liable, than you would have been otherwise, to fall into the very common error of confounding cases of this description with those of real local disease. This is the principal object which I have had in view, in directing your attention to this subject; but it is one of much interest, and I am unwilling that you should leave it without proceeding somewhat further in the enquiries to which it leads. In the present lecture, then, I propose to offer some observations on the pathology of these cases, and on the treatment which should be employed for their relief.

Probably the following question has already presented itself to your minds. Is there any sufficient evidence that symptoms so various and dissimilar, as some of those which have been described, depend on one and the same cause? Are there good grounds for the hypothesis that a pain in the knee in one case, retention of urine in a second, tympanitis in a third, are only different manifestations of one and the same disease, and that they are connected with the same state of system as that which gives rise to the common fits of hysteria? The same question may arise if you refer to Sydenham's observations on hysteria, in which he has endeavoured to point out the symptoms which may mislead the medical, as I (following him *haud passibus æquis*) have now endeavoured to point out those which may mislead the surgical practitioner. To this it may be answered, that there is scarcely a single case, such as I have endeavoured to describe, in which, if you have the

opportunity of studying its history and progress, you will not find abundant proof of the patient having suffered, in a greater or less degree, from the ordinary and acknowledged symptoms of hysteria; the two orders of symptoms sometimes existing simultaneously; at other times, and more frequently, alternating with each other; and thus even a limited experience will enable you to satisfy your minds on the subject. But when you have attained an enlarged experience in your professsion, you will find that it affords you evidence of another kind, though of such a nature that one individual cannot well communicate it to another, either in a lecture or writing. You will then find, that while no two of these cases are precisely and in all respects alike, it is by no means difficult to trace a series of cases leading from one to the other by an almost imperceptible gradation, and connecting with each other symptoms which, in the first instance, might be regarded as the most distant and heterogeneous.

Another question cannot fail to arise in the progress of these investigations. What is the real nature of the disease on which these various and anomalous symptoms depend? We cannot doubt that its locality is in the nervous system. This is sufficiently demonstrated by the character of the symptoms themselves. Dissection, which illuminates so many of the darkest regions of pathology, affords us little assistance here; at least we derive from it only negative information. I have, in several instances, examined the parts to which hysterical pains had been referred; and, in one very aggravated case of the kind, I made a careful dissection of all the nerves by which they were supplied; but I have never been able to discover in them any thing different from what belonged to their natural condition. But every part of the body has its corresponding point in the brain, and the greater number of them have their corresponding points in the spinal chord also. Does the examination of these organs lead to any more satisfactory result? The best proof that it does not do so, is furnished by the following circumstance:—although so many die of other diseases, who have suffered from hysteria also, and the opportunities of examining the bodies of hysterical patients after death are therefore sufficiently numerous, yet the works of the best morbid anatomists contain no observations whatever on the subject. I have had the opportunity of instituting *post-mortem* examination in three cases, in which the hysterical affections were of so aggravated a kind as to be, directly or indirectly, the cause of death; and you shall know the result. In one of them, the patient laboured under a very severe hysterical pain in the side, and was liable, among various other hysterical symptoms, to fits, in which she was scarcely conscious of her own actions. It must have been in one of these attacks that a great number of needles were introduced into one of her legs, which afterwards occasioned much inflammation and effusion of serum into the cellular texture. The patient died, and the body was most carefully examined, but

no morbid appearances of any kind could be discovered in it, except what belonged to the œdematous state of the leg. Another case is one to which I have referred already, in which, the patient having long laboured under an hysterical retention of urine, the bladder was found enormously distended, of a black colour, the mucous membrane and muscular tunic being, at the same time, much attenuated. This patient was an unmarried female, twenty-nine years of age. Having been previously indisposed for a considerable time, she was supposed to have sprained her wrist in lifting a heavy saucepan. From this time she was never free from pain, in the situation of the outer part of the lower extremity of the radius. The pain extended up the fore-arm, and downwards on the side. In November, 1814, about a month after the occurrence of the accident, she was admitted into the hospital. At this time, the most careful examination could detect no alteration in the appearance of the limb, but she complained of a constant and intense pain, which extended from the supposed seat of the injury downwards to the fingers, upwards to the shoulder, and again downwards to the spine and sternum. She had great oppression and difficulty of respiration, occasional twitches of the muscles of the face, and any sudden motion of the hand aggravated all these symptoms, and then threw her into a state approaching to that of syncope; in which she was almost unconscious of all that happened, lying with her eyes wide open, and at last recovering with an hysterical sobbing. Her pulse was feeble, beating one hundred and twenty times in a minute. Forty ounces of urine were drawn off from the bladder, but without any relief as to the other symptoms. The tongue became black and dry; the pulse more feeble; the belly tympanitic; the alvine evacuations being of a dark colour. Then there was hiccup and vomiting; she became weaker and weaker, and died after the lapse of fourteen days from the time of her admission into the hospital. After death, the brain and the thoracic and abdominal viscera were very carefully examined, but no morbid appearances were discovered in any one of them, with the exception of the peculiar condition of the bladder, which was described formerly, and two ulcers of the mucous membrane of the *ileum*, each not more than half an inch in length, but occupying almost the entire circumference of the intestine.

The female, who was the subject of the third case, had laboured under a paralytic affection of the lower limbs (*paraplegia*), which Dr. Seymour believed, with good reason, to be connected with, and the consequence of, hysteria. A practitioner whom she consulted, however, thought it advisable to have recourse to repeated bloodletting and other methods of depletion. The result was, the formation of extensive sloughs of the nates and of the soft parts covering the ankles. The patient was now admitted into the hospital, in a state of great exhaustion, and soon afterwards died. The brain and spinal chord were most carefully examined, in the presence of many of you who are now present, but it could not be

discovered that they differed, in the smallest degree, from their natural condition; nor were there any signs of disease in the thoracic or abdominal viscera.

In adducing these facts, however, I by no means intend to assert that the organisation of the nervous system, in a person who is liable to aggravated hysterical affections, differs in no respect from that of another. The intimate structure of the brain, spinal chord, and nerves, is on too minute a scale for our senses to be able to perceive and comprehend it, and of course there may be differences in the organisation of these organs which our senses are incapable of detecting also. There is, it is true, nothing in the history of hysteria to justify the opinion that it is connected with any morbid growth, or morbid change of structure, such as we find to exist in what are usually termed organic diseases; but it is easy to suppose, without reference to organic disease, that the construction of the nervous system, at the period when growth is concluded, may not be the same in all individuals, and that an imperfect development of it may lay the foundation of all the aggravated hysterical affections. It seems to me that this hypothesis affords a reasonable explanation of the phenomena which those strange diseases present to our observation, and that it is not easy to explain them in any other manner. This being admitted, the connection of hysteria with the habits of early life, while growth is going on, becomes no mystery. We can understand, also, wherefore it is that the disposition is often, to a certain degree, hereditary; that it prevails in particular families, and that having been once established in the system it is never totally eradicated. Nor is this opinion in any way contradicted by the circumstance of hysterical symptoms alternating with longer or shorter intervals of perfect health. It is the same with many other nervous diseases, some of which are much more formidable than these. The lunatic has intervals in which his delusions vanish. A tumour pressing on the brain may occasion epilepsy; the cause exists always, but after the patient has had one fit, weeks or months may elapse before he has another. In like manner, a patient may have a nervous system so constructed as to render her liable to attacks of hysteria. While she is strong and healthy in other respects, no hysterical symptoms arise; but if she be weakened by an attack of fever, by loss of blood, by too great exertion of mind and body, or depressed by anxiety, grief, or disappointment, the disease is rendered manifest, and it assumes one form or another, accordingly as accident directs its influence to one or another part of the system.

This view of the origin and nature of hysterical affections derives some confirmation from a circumstance which I have had frequent occasion to observe; although it has not, as far as I know, been noticed by pathological writers. In those who are much disposed to them, there is an evident weakness and laxity of the tissues, independently of what may be supposed to belong to the tissues of the nervous system. Thus there is a peculiar looseness

of the joints; sometimes existing to such an extent that they are liable to a kind of subluxation (a slipping in and out, as the patient terms it), without any laceration of the synovial membrane or ligaments. I have known several cases in which a patient, on making some sudden exertion, has experienced a sensation as if some muscular or ligamentous fibres had given way; and, in some instances, a severe nervous pain, referred to this and the neighbouring parts, has remained for a long time afterwards. It is not unusual for the smaller blood-vessels to burst, so as to occasion slight hemorrhage, although there is no actual disease in the bleeding part. This occurs most frequently with respect to vessels of the mucous membranes. The disposition to hemorrhage, however, is not peculiar to these textures. In a patient concerning whom I was consulted with Mr. Mawdsley, there had been repeated hemorrhages from the ears.

These things must be regarded as indications of want of physical power in the system, and such is the prevailing character of hysterical disease; most distinctly marked, of course, in the most aggravated cases of the kind. A large proportion of hysterical patients suffer from cold hands and feet, have a feeble contracted pulse, a small appetite for food, and are wearied by very small exertions; they are more liable than other persons to lateral curvature of the spine. In some instances, and more especially in the parts which are most exposed to the external temperature, or at the greatest distance from the vital organs, the point of the nose, for example, and the ankles, the circulation is so weak that they assume, at times, a purple appearance, followed by vesications, and even by a thin slough. These last-mentioned symptoms are, in themselves, a proof of an insufficient generation of nervous energy; they correspond to what is observed after severe injuries of the spinal chord, as well as to what occurred in the following cases, as the consequence of an injury of the nerve. A young man met with an accident, in which the ulnar nerve was divided behind the inner condyle of the arm. The wound healed readily; but when I was consulted, about three months afterwards, the little finger was cold and deprived of sensation, with purple spots upon it, similar to those which precede the formation of vesications. A girl was admitted some years ago into the hospital after a similar accident. The little finger was cold and benumbed, and occasionally the whole of the integuments covering it assumed a dark purple colour; this was always followed by a broad vesication; then by a superficial sore, which, however, healed by the formation of a new cuticle; and this process was repeated several times while the girl remained in the hospital.

In some instances, the disposition to hysteria manifestly depends on an original malconstruction of the nervous system, which probably has been transmitted from the parent to the child; in others it is equally manifest that it is the result of injudicious management in the early part of life. In the latter order of cases, the ill consequences which would otherwise ensue, may be altogether averted

by the timely adoption of a better system of education; and in the former, much may be done in the interval between the period of infancy and that of growth being completed, to improve the condition of the individual, and to render her situation in after-life less distressing than it would be otherwise.

You can render no more essential service to the more affluent classes of society than by availing yourselves of every opportunity of explaining to those among them who are parents, how much the ordinary system of education tends to engender the disposition to these diseases among their female children. If you would go farther, so as to make them understand in what their error consists, what they ought to do, and what they ought to leave undone, you need only point out the difference between the plans usually pursued in the bringing up of the two sexes. The boys are sent at an early age to school, where a large portion of their time is passed in taking exercise in the open air; while their sisters are confined to heated rooms, taking little exercise out of doors, and often none at all, except in a carriage. Then, for the most part, the latter spend much more of their time in actual study than the former. The mind is over-educated at the expense of the physical structure, and, after all, with little advantage to the mind itself; for who can doubt that the principal object of this part of education ought to be, not so much to fill the mind with knowledge as to train it to a right exercise of its intellectual and moral faculties, or that, other things being the same, this is more easily accomplished in those whose animal functions are preserved in a healthy state than it is in others?

But these observations relate only to measures of prevention; whereas, in practice, you will have to deal with cases in which the hysterical construction of the nervous system already exist.

The medical treatment of hysteria is in the department of the physician; and as this subject is treated of at length in the lectures on the practice of medicine, I shall only offer a few observations as to the principles on which it should be conducted.

In those in whom the liability to hysterical diseases exists, as I have already had occasion to observe, the symptoms of hysteria are not always present, and much may be done by art towards rendering their occurrence less frequent, and their character less severe, than would be the case otherwise. These symptoms are especially called into existence whenever, from any cause, the bodily powers are reduced below the ordinary standard; and it is reasonable to suppose that an opposite effect will be produced by whatever tends to alleviate these powers, and maintain the general health. The whole class of tonic remedies, especially steel, quinine, sulphate of zinc, and ammonia, may, under certain circumstances, be employed with advantage. So, also, it is of importance that the patient should live on a generous diet; that she should take exercise out of doors; that she should live in the pure air of the country rather than in that of a crowded city; and

that her mind should be agreeably occupied, without being exhausted by great exertions. Nothing tends more to aggravate the disposition to hysteria than the tedium and *ennui* of a life without occupation; when the mind is, as it were, thrown back upon itself, brooding over imaginary misfortunes, and creating for itself objects of anxiety.

The use of what are usually called antispasmodic remedies, especially valerian and asafœtida, is indicated, not where there is merely a liability to hysterical symptoms, but where these symptoms are actually present. Those tonics which are useful in preventing these symptoms are useful in the removal of them also, especially where the disease assumes a chronic form, as it generally does in the cases which fall under the observation of the surgeon. Here, also, I have in several instances known much advantage to arise from a long-continued course of the sulphate of copper, administered in pills, in small doses. Nor must we overlook another important rule of practice. There is often some particular circumstance in the state of the system at the time, which operates as the immediate exciting cause of the hysterical symptoms, and which medicine may remove. For example, in one individual there may be a furred tongue, and a costive state of the bowels; in another, deficient menstruation; and purgatives and emmenagogues may be administered with advantage, either separately or in combination. Again, it is not unusual in aggravated cases of hysteria to find the urine depositing a large quantity of lithic acid, in the form of sand; or the urine may be voided high-coloured, depositing a pink amorphous sediment, abounding in the lithate of ammonia; and in either of these cases the exhibition of alkalies, combined with alterative doses of mercury, purgatives, and a regulated diet, will contribute to produce a cure, the unhealthy quality of the urine seeming to be the cause, rather than the effect, of the hysterical affection.

On all these points I refer you to the instructions which you will receive from some of your other teachers; but there are some questions connected with the surgical treatment of local hysterical affections, into the consideration of which I shall feel it my duty to enter more fully; although, in so doing, the advice which I shall have to give you will be for the most part of a negative kind, relating not so much to what you ought to do as to what you ought to leave undone.

Hysterical pains are sometimes relieved by friction with a stimulating liniment; such, for example, as the compound camphor liniment, which may also be used in combination with the tincture of opium. The application of the belladonna plaster is occasionally useful, although it does not produce those remarkable effects which not unfrequently follow its use in other cases of neuralgia.

Hysterical pains are sometimes palliated by bathing the affected part with the following lotion, applied tepid:—

R. Misturæ camphoræ, ʒ iss; spiritus rosmarini, ʒ iss. M. Fiat lotio.

In some instances, the patients derive advantage from the exposure of the part to the vapour of hot water. This is especially useful in the cases of that peculiar affection of the wrist and hand, which I described in the last lecture.

In those cases in which the limb to which the symptoms are referred is affected alternately with heat and cold, I have known the following plan of treatment to be attended with excellent effects. During what may be termed the hot fit, let a compress be applied wet with cold spirituous lotion; and when the heat has subsided, and the limb has become cold, let a thick woollen or worsted stocking be drawn over it, and then an oiled silk covering over the stocking, so as to confine the heat and perspiration. When the cold fit has subsided, the oiled silk covering may be removed. This local treatment, however, should be combined with the exhibition of the sulphate of quinine, the use of which seems to be especially indicated by the intermitting character of the symptoms.

In some cases of hysterical neuralgia the patient is supposed to derive benefit from the abstraction of blood by leeches, or cupping, or even by venesection. Indeed, I have no doubt that the loss of blood is occasionally followed by a real alleviation of pain. But the relief is never otherwise than temporary; and wherever I have known this kind of treatment to be frequently resorted to, the ultimate result has been, certainly, not only not beneficial but absolutely injurious to the patient. In fact, we may lay it down as a general rule, that whatever lessens the physical powers tends to prolong the duration of hysterical diseases of all kinds; and nothing produces this effect in a more marked manner than repeated blood-letting. Those who are subjected to this treatment, according to my experience, become almost invariably invalids for life; and I have no doubt that not unfrequently their lives are materially shortened by it.

Blisters, issues, and the whole class of counter-irritants, in the majority of cases, increase the patient's sufferings; and there is one objection that may be urged against all local remedies, which applies especially to these, namely, that they prevent the attention being abstracted from the local symptoms. I may take this opportunity of observing, that nothing is more essential to the patient's recovery than that her mind should not be constantly occupied with the subject of her ailments. The treatment employed should be such as will involve as little as possible deviation from the ordinary habits of life. Thus in a case of hysterical neuralgia of the knee or hip, it seldom happens that any real amendment takes place while the patient remains confined as an invalid to her sofa. The pain may abate, but a sense of weakness follows, which disables her from walking more than the pain itself, and which, for obvious reasons, goes on increasing in proportion as the confinement is of longer duration. The first step towards a cure is, that

she should have sufficient strength of mind to begin to use the limb in spite of present suffering.

Another question connected with surgical practice remains to be considered. In hysterical diseases affecting the extremities, will any advantage arise from the division of the nerves which supply the affected part, so as to destroy the communication between it and the sensorium? or from the entire removal of the part itself, by excision or amputation? If the view which I have been led to take of these affections, namely, that they belong to the nervous system generally, and not to the part to which the symptoms are referred, has any foundation in reality, it cannot be expected that such operations will lead to any good result; and the notorious failure of similar operations, when performed in cases of *tic douloureux* of the face, and *tetanus*, undoubtedly tends to confirm this opinion as to their utter inutility. Pathological science, however, is not so far advanced as to authorise us in any instance to disregard the lessons of experience; and it is well, before we arrive at a positive conclusion on the subject, that we should refer to this higher source of instruction.

In a case, which I have already mentioned, of a young lady who had a train of most severe hysterical symptoms following the accidental prick of her finger, I was induced (many years ago) to divide the digital nerves. This was affected by a circular incision, carefully performed, extending through the whole of the nerves, integuments, vessels, and cellular texture, to the bones laterally, and to the aponeuroses of the tendons, anteriorly and posteriorly. The result was, that the patient's sufferings were aggravated rather than relieved.

As long ago as the year 1818, I was requested to visit a lady in the country on account of a disease of the knee. I was led to believe that she had laboured under an inflammation of the synovial membrane, which had in a great degree subsided, but that the harder textures had suffered in consequence, and that the cartilages were in danger of being ulcerated; and I recommended a plan of treatment accordingly. Whether, with my present experience on the subject, I should have taken the same view of her case, I will not undertake to say; but the result was, that a material improvement took place in the first instance. After some time, however, there was a manifest aggravation of all her symptoms; she suffered more than ever, so that she became anxious to undergo the amputation of the limb. I was now again consulted respecting her; but, from the written accounts which I received, I concluded that the pain did not indicate the existence of any serious disease, and that the circumstances of the case did not justify so violent a measure as had been proposed. However, her wishes remained unaltered; and two surgeons of eminence in the country, yielding to her entreaties, performed the operation. On dissection of the amputated joint, they were surprised to find that there was no collection of matter in its cavity; that the cartilages

had disappeared in one spot, of very limited extent; and that there was no other mischief. The stump healed readily enough, but she obtained no relief. I had the opportunity of seeing her some months after the operation, suffering more than ever, with intense pain in the stump, and violent convulsive action of the muscles which move the thigh bone on the pelvis.

Mr. Soden, of Bath, informed me of another of these cases, which fell under his observation, in which also the limb was amputated above the knee, but with no better result than in the case last mentioned. The symptoms attacked the stump, and the patient suffered as much after the operation as she had done before.

The history of a third case of the same kind has been published by Mr. Mayo, in his Outlines of Pathology. The knee was amputated, and the stump healed. Soon after the stump was accidentally struck, and this slight accident was followed by pain in the part, exactly similar to that which had been referred formerly to the knee. Amputation was then performed a second time; but as the wound healed the pain recurred, being again referred to the stump. Mr. Mayo then divided the sciatic nerve, below the edge of the *glutæus maximus* muscle. At first the pain was supposed to have been relieved, as after the former operations, but it returned on the wound being healed. At this period I had the opportunity of seeing the patient—the pain which she endured being as severe as ever. In short, she had undergone these various operations, without having derived the smallest advantage from any one of them.

It must be acknowledged that these, and other similar cases which might be enumerated, seem to be quite conclusive against all attempts to relieve these hysterical affections by an operation. Some evidence, however, may be, and has been, adduced on the other side of the question.

A young woman was bled in the arm, in July, 1820. The wound healed as usual; but on the 7th of August she was admitted into St. George's Hospital, labouring under hysterical pain, referred chiefly to the cicatrix, but extending also downwards to the hand, upwards to the axilla, and again downwards on the side to the leg and foot—the latter being at the same time in a great degree benumbed. The whole of the arm was cold, and of a purple colour, and the skin was exquisitely sensible when pinched. On the 25th of August I excised the cicatrix. She was supposed to be immediately relieved; and when the wound made in the operation was healed, she left the hospital as cured. So far, then, it appeared as if the operation had been successful; but observe what happened afterwards. At the expiration of two months she was re-admitted, not on account of a recurrence of the pain in the arm, but with other symptoms depending on the same state of the general system. The nose was cold and of a purple colour, and there was a similar condition of the integuments of the ankle. On the latter there was a broad vesication, and both of these parts seemed as if on the point of becoming gangrenous. This result, however, did not take place, and I lost sight of the patient some time afterwards.

In Mr. Mayo's patient,[1] whose case I have already mentioned, we are informed that he afterwards was induced to perform a further operation—removing the head of the thigh bone from the acetabulum; and I have a letter from Mr. Mayo, in which he states that this last measure has been followed by a relief from pain up to the present time. We are also informed that Sir Astley Cooper amputated the arm at the shoulder joint, on account of a neuralgic affection of a stump, and that the patient was permanently cured; and that a similar operation was performed successfully by Mr. Bransby Cooper. However, until we know more of these cases than is now recorded, it is impossible for us to determine whether they did or did not belong to the class of hysterical affections. Even if they did, the question still remains: how long did the patients remain under the observation of the surgeons afterwards? —and was a cure really obtained, or was there simply a commutation of one hysterical affection for another?

In estimating the value, not only of such operations, but of various other modes of treatment which have been supposed at one time or another to be useful in cases of aggravated hysteria, we are never to lose sight of the following circumstances:—
1. *Hysterical symptoms frequently disappear at once, without any manifest cause for their disappearance.* Examples of this fact may be found among the cases to which I have had occasion to refer in the preceding lectures. A young lady, who had been for more than two years confined to the recumbent posture on account of an hysterical affection simulating disease of the hip joint, recovered suddenly one night while in the act of turning in bed. Another young lady, in whom a long train of most severe hysterical symptoms followed an accidental prick of one of her fingers, after the disease had existed for a great length of time, (if I am not much mistaken, for more than two years,) recovered also.
2. *It still more frequently happens that recovery from hysterical symptoms immediately follows a forcible impression of any kind made on the nervous system.* Hence it is that any thing may obtain the credit of having effected a cure in these cases. Moral and physical agents are alike in this respect. Sometimes one remedy may appear to be successful, sometimes another; and that which is supposed to be productive of the greatest benefit, in one case, may never be useful afterwards.

I have already mentioned the case of a young lady who, having long laboured under an hysterical neuralgia of the hip and thigh, rendering her unable to stand, or even to walk, immediately lost all her symptoms on being thrown from a donkey which she was riding; and the following are only a few among many other cases which might be adduced in confirmation of what has been just stated.

In the eighth volume of the transactions of the Royal Medical and Chirurgical Society, Mr. Pearson has described the case of a

[1] Medical Gazette, May 7, 1836.

lady who laboured under a nervous affection of the hand and fore-arm, showing itself in the form of severe pain and spasms of the muscles; and she immediately recovered on the application of a stimulating liniment, which, containing oil of turpentine, produced a vesicular eruption over the whole person.

I was informed, on good authority, of the case of a young lady who had long laboured under a severe hysterical affection, attended with spasmodic contraction of the muscles of one of the lower limbs, and which symptoms left her suddenly, on the extraction of a molar tooth.

Many years ago I attended a young lady on account of a painful affection of the instep, which I certainly did not understand at the time, but of which, with my present experience on these subjects, I am satisfied that it was hysterical neuralgia, and nothing else. She was attended by other surgeons afterwards, who, I believe, were as much perplexed as I was, as to the nature of the disease, and who, at all events, gave her no relief. At last, while suffering as much as ever, she was informed of some remarkable cures obtained by the use of the vapour bath and champooing; and she immediately went to Brighton, that she might make a trial of these remedies. The first champooing gave her great relief; the second completed the cure. I was consulted respecting her afterwards, labouring under a nervous affection of the arm and fore-arm.

In the "Christian Observer" for November, 1830, we find recorded the case of Miss Fancourt, who had long been unable to move in consequence of what was evidently an hysterical affection, simulating disease of the hip joint, and was supposed to have been miraculously cured under the influence of the prayers of her spiritual adviser; leaving her couch at once, and walking down stairs to supper, to the astonishment of her family.

We need not pursue this part of our enquiries further. To you who will soon be engaged in the practice of your profession, what I have now stated will be sufficient to impress your minds with a proper degree of skepticism, and to prevent you being misled by the caprices of these strange disorders. With respect to the great majority of society, whose minds are not accustomed to these investigations, and who do not know the difficulty of obtaining exact evidence as to the operation even of the remedies in common use, I feel that it will be almost a waste of time to endeavour to enlighten their minds on the subject. They will always be disposed to listen to, and to believe, the histories of the marvellous cures of hysterical affections; and with them conjurors of all kinds, from Prince Hohenlohe and the professors of animal magnetism down to the most vulgar impostors, will always be the successful rivals of those practitioners who have studied their profession as a science.

Before I quit the subject, I shall trouble you with one further piece of advice. I have told you that it is most important that you should not mistake cases of nervous affection for those of real

local disease; it is equally important that you should not mistake the latter for the former. Whenever you are in doubt, be careful that you do not employ any kind of treatment which would be injurious if local disease existed. A short delay will always enable you to understand the exact nature of the case, so that you can no longer hesitate as to the remedies which are required for its relief.

THE END.

ON THE

SURGICAL TREATMENT OF DEAFNESS.

BY M. ITARD, OF PARIS.

The following extract from a memoir by M. Itard, which was conceived of sufficient importance, by one of the most learned bodies in Europe, to be published in their transactions,[1] is of practical interest, as indicating what may be permanently expected from the operative methods that have been long adopted in France; and, of late, more extensively in this country. M. Itard is an aurist of the first scientific knowledge, who has himself published a valuable Treatise on the Diseases of the Ear;[2] and who, with MM. Cornac and Nacquart, was appointed by the Académie Royale de Médecine to examine a memoir presented by M. Gairal, a young assistant surgeon to the 12th regiment of dragoons, entitled "Researches on Deafness, considered particularly as respects its causes and treatment;" as well as certain instruments of his invention for the performance of the principal operations undertaken for the cure of deafness. R. D.

The operations performed for diseases of the ear may be reduced to a small number. The two principal are perforation of the membrane of the tympanum, and catheterism or injection of the Eustachian tube. We shall confine our reflections to these two, as being the most important, and those only that are referred to in the memoir of M. Gairal. We shall consider them for a moment as regards their applicability and the results that may be obtained from them, terminating with a description of the operative methods and instruments proper to them.

PERFORATION OF THE MEMBRANE OF THE TYMPANUM.

There is but little agreement as to the indications that may require perforation of the membrana tympani. M. Gairal has multiplied them greatly: he has advised it in deafness caused by chronic inflammation of the cavity of the tympanum, and in dropsy and effusions of blood into the cavity; in thickening and ossification of the membrane, and in impenetrable occlusion of the Eustachian tube.

[1] Mémoires de l'Académie Royale de Médecine, tom. v., fascic. 4. Paris, 1836.

[2] Traité des Maladies de l'Oreille et de l'Audition. Par J. M. G. Itard. Tom. II. Paris, 1821.

Setting aside the case of chronic otitis, in which the operation can be of no avail; those of effusion of blood or serous fluid, of the existence of which we can have no certain sign, and that of ossification of the membrana tympani—too uncommon a degeneration to require a discussion as to its remedy; let us consider the cases of thickening of the membrane, and of obstruction of the tube, which have been supposed most favourable to the success of perforation.

Thickening of the membrana tympani has been pointed out by all the authors that have written on diseases of the ear, as an obstacle to its functions, but no one has described in what manner it can be detected: nor has M. Gairal. Generally, this morbid condition of the membrane is manifested by dull opacity, in place of brilliant transparency—a change, however, which may take place without any sensible augmentation of thickness. There are cases in which it is less equivocal, when the visible side of the membrane presents a fleshy, rugous, cellular, or striated appearance. Whatever may be the signs of thickening of the membrane, its consequences, as regards audition, are not of the importance that has been conceived. When it is simple, and without any organic disease of the ear, this slight hypertrophy cannot be regarded as a cause of deafness; and this for the following reasons:

When we examine both ears in those who are affected with deafness of one side only, the membrane of the sound ear exhibits a degree of thickening which does not appear to exist in the affected ear, or is less marked. Repeated attacks of otalgia frequently induce thickening without deafness resulting.

There is a fact of another nature, which favours our opinion, and which may be added to other evidences of the false application of the laws of vibration to the functions of the membrana tympani.

In accumulations of cerumen in the meatus auditorius, it often happens that deafness exists only on one side, although the meatus on both sides may be closed hermetically by cerumen. In the side, however, in which audition is not sensibly impeded, it is only the bottom of the meatus which contains a certain quantity of cerumen—say a line or two in thickness, and glued to the membrana tympani. How are those vibrations of the membrane, which are supposed indispensable to the transmission of sounds, effected through this soft and plastic medium?

From these practical observations we feel justified in concluding that thickening of the membrana tympani, when it is the only lesion of the meatus, does not induce a sufficient difficulty in the auditory functions to require perforation of the membrane; and that the operation must be still more useless in cases in which the thickening is accompanied by deafness—inasmuch as the material cause of the latter must be seated elsewhere than in the membrane.

Let us pass to the occlusion of the Eustachian tube, which has been regarded as the cause of another kind of deafness, also curable by perforating the membrana tympani. Such an advantageous result might doubtless be anticipated, if the impervious

state of the tube were the only cause of deafness; but it is presumable that, in those cases in which the tube is completely and irretrievably closed, the morbid cause which has produced the closure has extended its influence to the other canals and cavities of the internal ear; and also, perhaps, that the artificial opening made into the cavity of the tympanum, on the side of the meatus auditorius, may not be an apt substitute, in all its functions, for the natural opening in the throat. Certain it is, that nothing is more uncommon than for deafness to be cured by perforation of the membrane alone.

In 1821, the reporter of your commission,[1] who had frequently performed it in the most favourable cases, could refer in his work to but one fortunate case, and it was afterwards followed by relapse. Since that time, in despair of the cause, but at the instance of the interested persons, he has again tried the operation in thirteen cases of imperviousness of the tube, and constantly without advantageous and permanent results.

Ought we to deduce from all this that the operation should be abandoned? Certainly not: it should be retained, were it only to facilitate the immediate application of certain agents to the internal ear—as when it is desirable to throw injections into it, which the occlusion of the tube renders impracticable; or, this channel being free, to communicate a powerful action to those injections, which thus proceed through and through, sweeping every thing before them. By means of this energetic auxiliary, perforation of the tympanum may be a successful operation, if properly adopted—as in strumous catarrhal obstructions of the tympanum, of which a remarkable example is to be found in the monograph already cited. But, employed alone and simply for opening a passage for the atmospheric air, this operation can only disappoint the best founded expectations of theory.

CATHETERISM OF THE EUSTACHIAN TUBE.

Catheterism of the tube occupies a very extensive space in the memoir of M. Gairal, and is treated carefully. This operation has evidently been the more particular object of his study and researches: the historical exposition of it is complete, and the judgment formed of the different operative processes leaves little to be desired. But we cannot say as much of his therapeutical appreciation of the operation. This essential point is only, as it were, glanced at, and what is said is vaguely disseminated through the articles on each species of deafness. We may be permitted to supply this hiatus, as yet completely untouched, notwithstanding the numerous memoirs that have been published for some years past on catheterism of the tube.

As catheterism of the tube, and the different therapeutical agents to which it serves as the medium of transport, do not reach beyond the middle ear, it may be laid down as a principle, that it cannot

[1] M. Itard himself.—*R. D.*

be advantageously employed in deafness, except the material cause of the infirmity be seated in this part of the organ; that is—in the tube, the cavity of the tympanum, or the mastoid cells. This material cause may consist in turgescence of, or excrescences from, the membrane that lines these cavities; in tumefaction of the osseous and cartilaginous parts that enter into their parietes; and, lastly, in mucous or purulent accumulations there.

Of these three kinds of obstacles, the two first—as they belong to the tissues, and produce contraction, obliteration, or deformity of the tympanic cavities—are not accessible to the advantages of catheterism. There remains only the third, which may consist of every kind of accumulation, more or less fluid, and more or less soluble and mobile, for which catheterism may be rationally invoked. But, in order that it shall succeed, it ought not to be regarded merely as a means for displacing or expelling accumulated matters, but as a method of medication for drying up their very source.

It is unnecessary to say that catheterism—properly so called—can neither fulfil the one nor the other of these indications. The operation can do nothing of itself; and, in the case even of narrowness or chronic engorgement of the pavilion of the tube, its action is always limited to ephemeral results. It is consequently, by therapeutical agents introduced into the ear by means of catheterism, that congestions of the tympanum may be dispelled, and their reproduction prevented. These agents consist in the introduction of water or air, with or without the addition of medicinal substances. Let us examine these two modes of medication under the names of liquid and gaseous injections.

Liquid injections have this advantage over the gaseous: that they wash out the mucus, and even the solid concretions, and are more easily charged with the properties of medicines that may be associated with them. The superiority of their detersive action is visibly exhibited in certain cases of otorrhœa, accompanied with destruction or spontaneous perforation of the membrana tympani. If an injection be made—not by the meatus auditorius externus, as above mentioned, but by the Eustachian tube—the liquid, which escapes at the concha with great facility, is turbid, or contains the matter of the discharge, and sometimes flakes of false membranes. This kind of injection is almost always followed by marked improvement in the deafness; but this is generally not of long duration. At times it is entirely dissipated, by adding some excitant medicament to the aqueous injection; but the great difficulty is to give to the action of the medicine a sufficient degree of energy to change the chronic into an acute, but moderate, inflammation. If we exceed this, the injury of the sense is aggravated. It would be too tedious, and too remote from our subject, to treat that important topic here.

We pass on to the second mode of medication practised on the middle ear by means of catheterism of the tube, which consists of gaseous injections.

The introduction of gaseous fluids into the surgical therapeutics of the ear dates from the beginning of this century. A memoir, inserted in 1816 in the fourth volume of the *Journal des Sciences Médicales*, testifies to the advantageous employment of gaseous injections into the tube, both with the aid of catheterism and without. Five years after the publication of this memoir the same system of treatment was revived, accompanied by fresh details and fresh facts, in the *Traité des Maladiès de l'Oreille et de l'Audition.* All these gaseous injections or insufflations had as basis, or rather, as excipient, atmospheric air, charged with certain medicinal substances in the state of smoke, vapour, or gas. We may add, also, as a necessary supplement to this historical elucidation, that insufflations of atmospheric air are recommended there, not only as a means of cure, but of diagnosis for the detection of catarrhal turgescence of the tympanum. (Vol. ii., p. 211.)

The apparatus for insufflation, described there, consists of a bottle, the bottom of which contains a small quantity of a very vapourisable medicinal agent; the mouth being so arranged that it can be adjusted to the pavilion of the sound, and furnished with a stop-cock, which is closed, and the vessel placed in boiling water. In a few seconds the heat rarefies the air, and volatilises the contained liquid; the stop-cock is now opened, and a jet of vapour escapes with a hissing noise, and is conducted by the catheter into the ear. In certain cases the operative procedure is yet more simple. A few drops of a spirituous tincture, also very vapourisable, are dropped on a piece of sugar, which is rapidly chewed; and then, by the aid of a forcible expiration, at the same time closing the lips and nostrils firmly, the vapour, disengaged into the mouth, is forced into the Eustachian tube.

Should it be desirable to inject the vapour of certain medicinal vegetables, they may be burned in the bulb of a tobacco-pipe—the smoke be inhaled, as in ordinary smoking—which may then, in the manner just described, be forced into the guttural channel of the ear.

Such was the state of matters on this point of science, when, about ten years ago, M. Deleau succeeded in modifying the operation; or rather, to use an expression of our time, in reducing it to its most simple expression. He rejected the gaseous injections then known, and employed atmospheric air as the only material for his insufflations, to which he gave the name *douches*, or "dashes," of air.

The "dashes of air" (*douches d'air*), offered as a new method of treating and detecting diseases of the ear—and, in this double relation, accepted and rewarded by the *Académie des Sciences*—deserve, by the single fact of such distinguished approbation, to attract, for once at least, the attention of the Academy.

We shall not stop to examine what there is really novel in this slight innovation. Debates for priority are rather questions of persons than of science, and should be carefully avoided.

What is important to be established for the interest of the art, is

a correct appreciation of dashes of atmospheric air sent into the cavity of the tympanum, both as a means of cure and of exploration.

As a curative agency, when we have recourse to pathological and physiological induction, it is not easy to accord to atmospheric air injected into the tympanum the double property which, we have seen, is necessary in order that these kinds of agents shall be curative; and which consists in expelling the matter that has accumulated, and preventing its reproduction. Its expulsive action is nearly null; good sense would suggest so—and the experiments which we have made on the dead body confirm it. As for its therapeutical action on the tissues to restore them to the healthy state, and prevent fresh morbid secretion, can this be expected from an agent with which these tissues are constantly in contact?

As, however, such a decision, however well-founded in theory, may be negatived by experience, we have felt that, in placing before the Academy our opinion on this point, we were bound to support it by a number of facts. These have not been wanting. We can affirm that, in the space of seven years, commencing in 1828, few weeks have elapsed without your reporter having had, at least once, an opportunity for trying the dashes of air in cases of catarrhal deafness, or those suspected to be such. The less violence was done to the conscience of the experimentalist by these preliminary essays, as they were devoid of pain, without any serious inconvenience, and often called for by the parties interested themselves.

To introduce greater precision in the administration of dashes of air, and to obtain results susceptible of more rigid evaluation than are afforded by the use of a pump or bellows, we employed as an injecting apparatus a condensing machine, furnished with a manometer, by which we could regulate the force of the douche, and establish a ratio between the intensity of the agency and its results.

The following is an analytical summary of our experience, divided into five categories.

FIRST CATEGORY.

Number and nature of the facts.—Deafness without obstruction of the Eustachian tube, or buzzing; twenty-nine cases.

Treatment.—Dashes of air to the number of from five to ten, and of from three to seven minutes' duration; under the pressure of from a half to two atmospheres, and of the size of a millimeter.[1]

Results.—No effect in the majority of the cases. In very impressible persons, and especially in females, some slight giddiness and buzzing, which passed off in a few hours; frequently slight headache.

SECOND CATEGORY.

Number and nature of the facts.—Deafness without obstruction of the tube, as above, but accompanied by buzzing; fifty-two cases.

[1] About two fifths of a line, or 0.039 Engl. inch.—*R. D.*

Treatment.—Number, duration, and intensity of the *douches*, same as the last.

Results.—Violent augmentation of the buzzing, and consequently of the deafness; and, in a great number, vertigo. These slight symptoms were of but little duration, and did not extend beyond four or five days in those who were most affected with them. One only, a Hollander, who was very plethoric, was tormented by them for a month and a half.

THIRD CATEGORY.

Number and nature of the facts.—Deafness from catarrhal obstruction of the tympanum; one hundred and twenty-one cases.

Treatment.—Dashes of air similar to the preceding, in duration and size, but carried to the number of from fifteen to twenty-five, and under a pressure of from two to four atmospheres.

Results.—More advantageous in five sixths nearly of those operated on, and especially after the first two or three *douches*. In fifteen, the cure was complete; but at the end of a very short space of time, varying from a few hours to two months, and in spite of the continuation or resumption of the same treatment, the disease returned in the like degree. On two only was the restoration of the functions maintained, and one of these set out immediately after his cure for Brazil, his native country; and in the case of the other, a young lady seventeen years of age, the treatment took place simultaneously with the regular establishment of menstruation.

FOURTH CATEGORY.

Number and nature of the facts.—Deafness, with catarrhal obstruction of the cavity of the tympanum, and buzzing; forty-eight cases.

Treatment.—The same as for those of the last category.

Results.—In twelve, improvement or cure, which, in no case, exceeded three weeks in duration; in the other thirty-six, no effect, or an increase of the buzzing as well as of the deafness: violent coryza.

FIFTH CATEGORY.

Number and nature of the facts.—Deafness, with catarrhal or purulent obstruction of the cavity of the tympanum, accompanied with destruction or spontaneous perforation of the membrane of the tympanum; twenty-eight cases.

Treatment.—*Douches* of air through and through, of six or seven minutes' duration, of the size of three fourths of a line; under the pressure of three to five atmospheres, and to the number of from fifteen to twenty.

Results.—No effect in one half; in the others, amendment of a few moments' duration: in none did the matter contained in the tympanum or in the meatus auditorius externus appear externally, although the air escaped audibly through the aperture in the membrane.

From this very abridged recapitulation of a series of two hundred and fifty-eight cases of treatment by dashes of air, without any durable result, except in two cases of cure in which other causes concurred, the Academy will probably agree with us, that the action of atmospheric air injected into the Eustachian tube is incapable of effecting a permanent cure in lesions of audition—even in those that are dependent upon physical obstruction of the cavities of the middle ear. Had all our results been negative, we should not have dared to deduce such a conclusion; for, although we might always have failed, it did not follow that others might not have succeeded: but the mixed nature of our facts gives them quite another value. They evidently show that the operations were well executed and applied, as a certain number succeeded, and that it is in the nature of successful results, obtained by such means, to have only an ephemeral duration; a deduction which might have been drawn from the instability of the cures obtained by simple injections of warm water.

If the dashes of air have had more durable and complete effects in other hands; if they have really, and without illusion, restored hearing to some who have been deaf-dumb from birth; it is probable that they have owed their curative qualities to some potent auxiliary which has been forgotten to be mentioned.

We have seen to what extent the therapeutical effects of dashes of air are available. We have yet to enquire into the diagnostic advantages which have been attributed to them, and admitted by a learned association. These advantages would seem to be—to indicate the pathological condition of the tympanum, by the noise caused by the forcing of air into it. The author, who first observed and described catarrhal deafness—one of the most frequent and curable forms—has long pointed out, as an ordinary sign of this cophosis, the sudden changes at times impressed on audition, either by sneezing, blowing the nose, or by any forcible and prolonged expiration, which drives a part of the expired air, retained in the mouth, violently into the tympanum.

Dashes of air, employed as explorers of the condition of the cavity of the tympanum, are, as we have seen, merely an extension of this last process; and, we may add, an abuse of its consequences, if we regard the sort of crepitation caused by the dash of air as a pathognomonic sign of obstruction of the cavity of the tympanum.

It would be very important to show that this phenomenon is in a constant ratio with such obstruction, and, as has also been affirmed, with the progress of the treatment. We may affirm that it is solely referable to the presence of mucus in the Eustachian tube, and in no respect to obstruction of the tympanum. This can be easily proved on the dead body; as the sign is in no respect vital, and being entirely dependent upon an invariable acoustic effect, the consequences deduced from our experiment cannot be questioned. It consists in filling the tympanum with nasal mucus through the meatus auditorius, and destroying the membrana tympani, which is afterwards replaced by a plug of suet. A strong and continued jet

of air is now directed, as in the living body, into the Eustachian tube, by means of the hollow sound or catheter. It is then found that, so long as the mucus does not reach the Eustachian tube, insufflation occasions no sound; and that, to produce it in all its intensity, it is but necessary to cause a part of the mucus introduced into the tympanum to run into the tube; or, if the cavity be left free, to deposite a little mucus in the bottom of the tube.

Under these circumstances, crepitation continues until the mucous matter which produced it has been driven into the tympanum by the current of air, or removed by reflowing towards the orifice of the tube, out of the direction of the column of air.

It must be observed, again, that this continued projection of air into the tympanum has no other effect on the contained mucus than to apply it against the parietes, and to crowd it towards the mastoid cells, without driving the smallest portion of it out. These mechanical results from the *douche* of air may be rendered still clearer by experimenting on the ear of the ox, in which the cavity of the tympanum, and especially the Eustachian tube, are of greater extent than in men.

From the results of these experiments, it may be concluded:

First. That, as a means for exploring obstructions of the tympanum by the crepitation which often accompanies them, *douches* of air can afford no certain index.

Secondly. That, as a mechanical agent for deterging or evacuating obstructing matters, they can only dislodge and force them together; and, consequently, that both in the second and first point of view, *douches* of air do not deserve the confidence of the profession.

[After criticising some of the surgical inventions proposed by M. Gairal, M. Itard proceeds.]

It affords us real satisfaction to be able to speak favourably of those of M. Gairal's labours and instruments which relate to catheterism of the Eustachian tube. On this point, this young surgeon may be said to have rendered a service to the science; for, by means of the sound, which he has invented, he has given to the chief operation in acoustic therapeutics a degree of perfection which it had not previously attained. It was especially with this object, as we have remarked, that his efforts and researches were directed, and proceeded in methodically. He first of all thoroughly studied the anatomical arrangement of the nasal fossæ and fauces, in order to calculate, with accuracy, the obstacles and facilities to be experienced in passing to the bottom of the tube a sound of this or that character, having this or that curvature, and formed of metal or some elastic material. Thence arose the necessity of examining critically every instrument and every variety of catheterism adopted in succession since the year 1724, the time at which this operation was thought of by a *maître de poste* of Versailles, who experimented on himself.

It would occupy too much time and space to follow M. Gairal in this review. We shall confine ourselves to mentioning the incon-

veniences which, in his view, are presented by the three operative processes now most commonly followed: those of the late M. Saissy, M. Deleau, and the reporter of your commission.

M. Gairal finds three inconveniences in the method of M. Saissy. First, it requires two different sounds, one for each nostril; secondly, neither can be used for injecting the tube through the opposite nostril, when that of the side affected is inaccessible; and thirdly, M. Saissy has laid down, as an indication of the arrival of the point of the sound at the orifice of the tube, the disappearance of the second curvature of the instrument under the ala nasi.

These defects are real, and the last especially is serious. They are an inevitable consequence of the fundamental and defective idea that guided the inventor of that catheter—which was to establish an exact relation between the three curvatures of the instrument and the anfractuosities of the canal it had to traverse. Now, as of all the cavities of the body, the nasal fossæ are those whose hollows, reliefs, and capacities present the greatest differences in individuals, it must result, that the relations established between the configuration of the nasal parietes and the shape of the instrument must be often defective, and render its application more or less difficult and inexact.

The objections urged against M. Deleau's method are more numerous and detailed. M. Gairal reproaches him for having, after the example of Cléland and Wathen, given a preference to flexible tubes, and for having affirmed that their introduction is more easy and less painful. He finds, on the contrary, that, in consequence of the very slight curvature given by M. Deleau to the beak of his sound, there is less facility than with any other in getting round the prominence of the inferior turbinated bone and pterygoid process, and, consequently, in entering the Eustachian tube; that, as respects pain, a sound of well-polished metal excites less than one of gum elastic, rendered stiff by a stylet, which must afterwards be withdrawn through the narrowest diameter of the nasal fossa. As for the principal end of the operation, which is gaseous or liquid injection, M. Gairal maintains that it is fulfilled less perfectly with a sound of gum elastic; which, in consequence of the greater thickness of its parietes, and its less calibre, can only furnish a small jet of fluid. M. Gairal might have added, that this jet, by the single circumstance of the elasticity of the canal which furnishes it, loses much of its impulsive force; and that, as another consequence of the flexibility of the canal, augmented by the moist heat of the throat, the beak of the sound remains loose in the pavilion of the tube, whence it is constantly pushed out by the reflux of the liquid or air. This has been demonstrated to us not only by experiments on the dead body, but also by observations on the living; for when we are desirous, in the latter case, of adding to the force of impulsion of the liquid or gaseous injection, it often happens that the extremity of the elastic sound is disengaged from the Eustachian tube, and the matter of the injection is thrown into the pharynx. This mobility of the sound is a serious inconvenience, which takes

from the operation its greatest advantage—that of forcing the strait of the tube when it is obstinately obstructed.

Proceeding to the examination of the sound employed and recommended by the reporter of your commission, M. Gairal does not find it less defective than the preceding. He criticises especially its too little curvature, which he thinks equally insufficient to embrace the projection of the pterygoid process, and of the inferior turbinated bone, and to attain the orifice of the Eustachian tube. He has almost demonstrated the impracticability of its being used at all for catheterism of the tube, in spite of the proofs we have daily furnished to the contrary for nearly five and twenty years!

The same impartiality that causes us to admit the justice of his observations against the two other operative processes, constrains us to agree that, in criticising the too little curvature of our sound, he has pointed out its capital defect; not that it is impossible to pass round the inferior turbinated bone, which is never necessary, provided the sound be passed beneath that bone—but because, when it has entered three or four lines into the pavilion of the tube, the beak of the instrument cannot be carried on to the bottom of the tube, which becomes necessary, as we have said above, in case of very tenacious obstructions, that close it completely.

In such cases, we employ a half flexible tube; that is, one, the straight part of which is of silver, and the neck or curved part of gum elastic. When the beak of this sound has once attained the orifice of the Eustachian tube, it readily glides as far as the narrow portion of the canal; and, supported by the metallic part, which is received into it, it holds its place against the retrograde impulse of the injected fluid. At times, however, there is a slight recoil in the soft portion of the tube, so that the substance of the injection escapes too readily between the orifices of the tube and sound. This inconvenience could only be removed by the use of an entirely metallic sound, with a bend sufficiently long to reach to the bottom of the tube, and yet not so long as to prevent it from executing the necessary evolution. This problem has been resolved in a very happy manner by M. Gairal, by giving his catheter a more decided and longer curvature, and by manœuvring it so that, at the moment the projection of the curvature is about to touch the inner paries of the nostril, the rest of the curvature is already received into the Eustachian tube. This conception, like all those that mark the progress of science, is simple and happy, and it is surprising it should not have occurred before.

The sound of M. Gairal is a silver tube six inches long, from one to two lines in diameter, and of a curvature of one hundred and fifteen degrees in an extent of two inches; and having on its convex surface numbers which indicate the distance to be passed over before it reaches the posterior aperture of the nasal fossæ. This distance being nearly the same as that between the dental arch and the loose margin of the velum palati, M. Gairal first measures the latter with a silver plate, which he terms a *palatometer*, on which is engraved a scale corresponding to that on the sound, and which

consequently indicates to what number the latter ought to be passed into the nostril. After having described in much detail the *modus faciendi* of catheterism, M. Gairal makes the following summary:

First. Measure with the palatometer the extent of the palatine arch. Secondly, Introduce the sound into the nasal fossæ, as far as the number indicated on the palatometer. Thirdly, Execute the movement of rotation as far as number 1 of the pavilion. Fourthly, Make it enter the tube by gently depressing the sound, and at the same time continuing the rotation as far as number 2.[1]

To explore the state of the Eustachian tube, and especially to detect the depth of obstacles that may exist there, M. Gairal has likewise invented a very delicate whalebone stylet, which, when introduced with the assistance of the sound into the Eustachian tube, may pass along the whole tympanum, and reach the vicinity of the ossicles of the ear. It would be difficult to indicate, *a priori*, the advantages and inconveniences of this method of dilatation and exploration. We must wait until experience has decided.

As regards M. Gairal's plan of, and his instruments for, catheterism, these tests at least have not been wanting. Our judgment has not been limited to the easy trials which we have seen made with it on the dead body. The reporter to your commission has embraced every opportunity, which his practice has afforded, to try them on the living. If we have not found the catheter of M. Gairal always of easy introduction, we can attest that its application has constantly appeared to us more exact and advantageous than that of any other sound; that if these advantages are of but little moment in the majority of cases, there are others in which they are decisive of success—as in obstinate obstructions of the strait of the tube; and others, again, in which a species of catheterism, not easily executed by other catheters, is rendered by them practicable and even easy. We would speak of that which has to be executed through the opposite nostril, when the nostril of the same side, owing to disease or defective conformation, does not admit of the introduction or evolution of the sound. We ought, therefore, to applaud the successful modifications which M. Gairal has made in the catheter and catheterism of the Eustachian tube; and, in consideration of the service rendered by him to a part of the science, still in not a very advanced condition, we have the honour to propose that the *Mémoire* of this young colleague should be deposited in your archives; that he should be thanked for his communication, and encouraged to pursue a career, the commencement of which has been marked with success.

[1] The "Report" is unaccompanied by plates.—*R. D.*

FORMULARY OF NEW MEDICINES.

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The earliest translation of the *Formulaire pour la préparation et l'emploi de plusieurs nouveaux médicamens*, by Magendie, was from the French of the third edition, by the editor's friend, Charles T. Haden, Esq., which, owing to the sickness of the lamented translator, was seen through the press by the editor of this work, who published a second edition, with numerous alterations and additions, in the year 1824; with an appendix, being a version of the additional matter in the fourth French edition of the original. These were reprinted in Philadelphia. Since then, different translations have been made from the successive editions by Houlton, Gully, Charles Wilson Gregory, &c.

The following epitome, from the *British Medical Almanac* for 1836, contains all the most valuable practical matter from these later versions, and from other sources.—*R. D.*

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PREFACE.

This formulary of new medicines, many of which are in general use, although they have not yet been introduced into the London Pharmacopeia, is abridged in great part from the eighth edition of Magendie's Formulary: authorities are often given for the employment of particular drugs; and where this is not done, general consent, or the opinion of M. Magendie, are the only sanction. A few additions have been made from Dr. Gully's translation; others have been gleaned from the medical journals. Some difficulty occurred relative to the weights, which may now be read as our apothecaries' weights.

The French grain is = 0.82 grains, or 5 French grains are nearly equal to 4 English grains; the *gros* is nearly 59 grains, (58.85,) or an English dram within 1 grain; the *scrupule* being the third of a gros (19.62 grains). In all cases the *gros* and *scrupule* have been rendered by dram and scruple; the difference was considered unimportant; and in other cases the quantities have been so altered as to preserve whole numbers, while the essential ingredient remained in the same dose. The liquids should be weighed; and it will be borne in mind that the French ounce is 471 grains, or 15 grains more than the English fluid ounce of distilled water.

NUX VOMICA AND ITS ACTIVE PRINCIPLES.

Alcoholic extract.—Dose: 1gr. to 5grs., and occasionally to 30grs., in pills, or till tetanic shocks are felt. Given in debility and paralysis of the system, or of a particular organ. (Chauffart, Edwards.) In dysentery. (Dr. Geddings.)

Tincture of Nux Vomica.—Alcohol at 36°, 1oz.; dry extract of nux vomica, 4grs.

Dose: a few drops in a mixture, or used in liniment.

STRYCHNIA.—Very pure strychnia, 2grs.; conserve of roses, ½ dram. Mix very accurately, and divide the mass into 30 pills.

Dose: one 15th to one 10th of a grain.

Tincture of Strychnia.—Alcohol, 36°, 1oz; Strychnia, 3grs. Mix.

Dose: from 5 to 20 drops, in any mixture or drink.

The sulphate, hydrochlorate, and other salts of strychnia, are more soluble, active, and poisonous than their base. Employed in the same cases as nux vomica. In hysteria, epilepsy, dyspepsia, gout, rheumatism, cholera. (Dr. Ryan.) In debility, paralysis from lead. (Dr. Copland.)

BRUCIA—discovered in the spurious angustura, (*brucea antidysenterica,*) and subsequently found with strychnia in the nux vomica.

Dose: ½gr. to 4grs. Paralysis. (Andral.) It may be used in tincture, or the more soluble salts may be prescribed.

OPIUM AND ITS ACTIVE PRINCIPLES.

MORPHIA.—The salts are generally used, as they are more soluble; their effect is less permanent.

Dose: ¼ to ½gr.

Solution of Acetate of Morphia.—Acetate of morphia, 13grs. Distilled water, 1oz.

Add 4 drops of acetic acid, and a dram of alcohol, to keep the salt in solution.

Dose: 6 to 24 drops.

The sulphate and muriate may be dissolved, and prescribed in the same form.

Solution of Citrate of Morphia. (Black drop?)—Take of opium, 4oz.; citric acid, 2oz.; to which add a pint of water, and macerate for twenty-four hours; then filter. (Dr. Porter.)

Or, pure morphia, 13grs. Citric acid, 6grs. Distilled water, 1oz. Tincture of cochineal, 2drs.

The dose is from 6 to 30 drops during the day.

The muriate and acetate of morphia are most generally employed in this country.

NARCOTINE.—To this Magendie attributes the exciting effects of opium; it is not given in medicine.

Extract of opium, *deprived of morphia or of narcotine,* may also be prepared. Narceine and meconine produce no appreciable effects.

CODEINE.—A whole year's experience of its use has shown me that a single grain (0.8) of codeine, given in two doses, generally produces a calm sleep, not succeeded, as is often the case with morphia, by lassitude and heaviness of the head. Less active than morphia, it should always precede it as a remedy. Patients, who had ceased to be affected by the salts of morphia, received great benefit from the salts of codeine. (Magendie.)

EMETIA OR EMETINE.

From the cephaelis ipecacuanha and viola odorata.

Four grains of *coloured emetine* will produce vomiting, and may be administered in the same cases as ipecacuanha.

Pure Emetine—Emetic mixture.—Pure emetine, dissolved in acetic acid, 1gr. Infusion of lime flowers,[1] 3oz. Syrup of marsh mallows, 1oz.

Take a large tea-spoonful (80 min.) every quarter of an hour, until vomiting is produced.

QUINIA, AND PRINCIPLES OBTAINED FROM PERUVIAN BARK.

Pelletier and Caventou obtained the *cinchonia* from the gray bark (*cinchona condaminea*); *quinia* from the yellow bark (*cinchona cordifolia*); the red bark yielded *cinchonia* like the gray bark, but in three times the quantity; and *quinia* in nearly double the proportion found in the yellow bark. Further experiments on a large scale have shown that both alkaloids exist in the three species of bark.

There are a sulphate and a supersulphate of quinia; the effloresced neutral sulphate is invariable, and the best for use. The dose, in intermittents, is 1 to 10grs. daily.[2] Magendie states, as the result of his experience, that 2 grains in twenty-four hours are sufficient to cut an ague short.

Quinia and Morphia.—Combinations of bark and opium have long been celebrated in intermittents.

Sulphate of quinia, 2 to 6 grains; Sulphate of morphia, ½ to 1 grain; divided into two, three, or four doses, may be conveniently substituted.

Quinia, with an emetic, has cured obstinate fevers. (Dr. D. Gola.)

Emetic tartar, 3grs. Sulphate of quinia, 10grs.

Mix, and divide into eight equal parts, of which one is to be taken every two hours during the intermission.

Quinia with Aloes.—Quinia, 2grs. P. aloes, 1gr.

Mix. It acts freely on the bowels. (Dr Copland.)

VERATRIA.

Obtained from the *Veratrum Sabadilla; V. commune; Colchicum autumnale,* and all the veratrum tribe: sabadilline and veratrine have also been found in the same family. Veratria is very acrid, and produces an abundant flow of saliva, violent sneezing, or diarrhœa. It is serviceable in procuring a prompt evacuation of the bowels, and in expelling accumulations of fæces in old persons. Colchicum and hellebore are uncertain in their strength and effects, and should give way to veratria.

[1] See Amer. Med. Intelligencer, vol. i. p. 38, for May 1, 1837.

[2] *Phloridzine,* obtained from the inner bark of the root of the apple and pear tree, is given in the same doses and in the same cases as the sulphate of quinine. (Van Mons.) See "Amer. Med. Intelligencer," No. 1, page 14.

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Pills of Veratria.—Veratria, 1 to 2 grains; gum Arabic and syrup of gum sufficient to make eight pills of each grain. One of these pills may be administered at first; and, if a purgative effect be not produced, three may be given in the course of the day.

Tincture of Veratria.—Veratria, 4grs. Alcohol, 1oz.

Dose: 8 to 20 drops in a cup of water. To be given in dropsy, anasarca, and gout, instead of tincture of colchicum.

Solution of Veratria (for *eau medicinale*).—Sulphate of veratria, 1gr. Distilled water, 2oz. and 2drms.

Dose: a tea-spoonful in one or two ounces of distilled water. I have frequently cured tic douloureux by producing small blisters made along the course of the nerves with a grain or two of veratria, repeating the operation every four or five days. (Magendie and Turnbull.)

Ointment of Veratria.—Veratria, 3grs. Lard, 1oz.

This may be used externally in chronic rheumatism, anasarca, and gout. (Turnbull.)

PRUSSIC OR HYDROCYANIC ACID.

Dose of the diluted acid, 1 to 4 min. Cures sympathetic cough, dyspepsia, with or without vomiting; pain of the epigastrium; pyrosis, and many affections of the chest (Dr. Elliotson); itching of the skin and acne rosacea (Dr. A. T. Thomson.) It is prepared in several ways: according to processes proposed by Scheele (employed in this country), Vauquelin, and Gay Lussac. Magendie adopts the last, which, when diluted with six times its volume of water, he calls *medicinal prussic acid.* The strength of the acid in London is very variable: Mr. Everitt found that samples from Allen & Co. yielded 5.8 per cent.; from Apothecaries' Hall, 2.1 to 2.5 per cent.; and from several other sources, only 1.4 per cent. Mr. Laming decomposes the cyanuret of potassium with tartaric acid, and, it is stated, thus obtains an acid of uniform strength.

Cyanuret of Potassium may be advantageously employed, instead of hydrocyanic acid.

Cyanuret of potassium, ʒi. Distilled water, ʒi.

It may be given in the same doses and cases as hydrocyanic acid, and should be prescribed extemporaneously.

Cyanuret of Zinc has been extensively used in Germany.

Cyanuret of Iodine and Hydrocyanic Ether have not yet been used in medicine.

IODINE

Is given in goitre, scrofulous ulcers, phthisis, and dropsy.

Tincture of Iodine.—Alcohol, at 35°, 1oz. Iodine, 40grs.

Dose: 4 minims to 20, three times a day.

Salt Iodated Waters of Lugol.—No. 1 (0.4), ½gr. in a pint of sea-water. No. 2 (0.5), ⅔gr. No. 3 (0.8), 1gr.

Children readily take these waters with sugar.

Magendie's formulæ are for *Iodureted Water.*

Ioduret of potassium, 6grs. Iodine, 1gr. Water, 2 pints 6oz.

This water may be the common beverage at meals.

Solution of Ioduret of Potassium.—Ioduret of potassium, 30 grains. Distilled water, 1oz.

These two preparations may be administered as the tincture. Magendie has found that the dose of the ioduretted solution of potass may be augmented to 2, 3, or 4ozs. a day, without inconvenience. Emaciated females have become stout during its employment. In young subjects with cardiac hypertrophy, the symptoms generally disappear in the course of a month, by taking large doses of the hydriodate of potass. (Magendie.)

Ointment of the Hydriodate of Potass.—Hydriodate of potass, 1 dram. Lard, 1½oz.

Ioduretted Hydriodate Ointments.

No. 1.	64grs.	Ioduret of potassium.	8grs.	Iodine.	1000grs.	Lard.
2.	160grs.	"	22grs.	"	1000grs.	"
3.	160grs.	"	26grs.	"	1000grs.	"

Ioduretted Sarsaparilla.—Decoction of Sarsaparilla, 2 pints. Ioduret of potassium, 1 dram. Syrup of orange-peel, 2ozs.

IODINE IN SCROFULOUS OPHTHALMIÆ.

Ioduretted Collyrium.—Rose water, 6ozs. Ioduret of potassium, 20grs. Iodine, 1 or 1½grs. (Coindet, Magendie, Lugol, Baudelocque.)

IODATE OF STRYCHNIA.—Dose: 1-10th of a grain, given with success in many cases of long-standing paraplegia.

IODURETS OF BARIUM and ARSENIC have been employed by M. Biett in ointments.

PROTO-IODURET OF MERCURY (yellow) *Ointment.*—Proto-ioduret of Mercury, 16grs. Lard, 1½oz.

In inveterate venereal ulcers.

DEUTO-IODURET OF MERCURY (red).—More powerful than the preceding. The ointment is made in the same proportions.

Alcoholic Solution of the Deuto-ioduret of Mercury.—Alcohol at 36°, 1½oz. Deuto-ioduret of mercury, 16grs.

The dose is 8, 15, or 20 drops, in a glass of distilled water. In scrofula, complicated with syphilis, it is said to be most successful.

Pills of Deuto-ioduret of Mercury.—Deuto-ioduret of mercury, 1gr. Extract of Juniper, 12grs. Liquorice powder, q. s. Divide into ten pills, two of which are to be taken morning and evening.

IODURET OF SULPHUR, *in ointments.*	No. 1.	No. 2.
Ioduret of sulphur,	5 parts,	8 parts.
Lard,	96 "	144 "

M. Biett has used ioduret of sulphur for several years in tubercular affections of the skin. Chronic Bronchitis. (Dr. Copland.)

IODURET OF ZINC, Dr. Ure proposes as a substitute for the ioduret of potassium.

IODURET OF IRON.—Dose: 2, 3, or 4grs. in distilled water, three

or four times a day; administered as a tonic in dyspepsia, strumous affections, chlorosis, amenorrhœa, tabes mesenterica, rickets, &c. &c. (Dr. A. T. Thomson.)

Iodure of Lead is one of the most useful preparations of iodine; it is milder than that of mercury.

BROMINE

May be given where iodine does not succeed, in similar cases.

CHLORINE.

Chloride of sodium and chloride of lime are extensively used to destroy fetid or impure odours, and have been called disinfecting agents. Chloride of sodium, in the proportion of one part to ten or fifteen of water, is applied to carbuncles, hospital gangrene, cancerous or ill conditioned ulcers (Magendie); corrosive tetter (Alibert); diphtheritic sore throat (Roche); and to burns, Lisfranc applies it from the commencement. The chlorides are recommended in fever by Stokes; in gonorrhœa, by Caussade. Chlorine is inhaled with advantage in phthisis, acute bronchitis (Gannal), and asthma.

MANNITE.

Dose: 2 drams, for children.

SOLANIA

Was discovered in *Solanum Nigrum* and *Solanum Dulcamara;* it produces vomiting and sleep.

DELPHINIA.

In Stavesacre (*delphinium staphisagria*) has the same medicinal effects as veratria. (Dr. Turnbull.)

GENTIANIA.

Tincture of Gentiania.—Alcohol at 24°, 1oz. Gentiania, 4grs. One of the best bitters in scrofulous affections.

LUPULINE.

Aromatic, tonic, and narcotic. (Dr. Ives.)

CROTON TIGLIUM.

The croton plant probably produces the Indian purging nut, which has the same violent action. (Recamier.) One hundred parts of the oil contain an acrid principle, 45 parts; fixed oil, 55. A violent purgative: applied externally in chronic rheumatism, (Ainslie,) and in inflammations of the thoracic and abdominal viscera.

Croton oil, 10 drops. Simple cerate, ½oz.

PIPERINE,

Found in black pepper, has the same febrifuge qualities as the cinchonic alkalies. (Dr. D. Meli.)

UREA.

Dose: 30grs. diuretic. (Magendie.)

LACTUCARIUM.

The juice of lettuce. Dose: 2grs. to 10grs., every twenty-four hours; narcotic.

GOLD.

The chloride of gold, the chloride of gold and sodium, the oxide of gold, the purple powder of Cassius, (probably deutoxide of tin and metallic gold,) are all useful in syphilis.

Frictions with the chloride of gold and sodium.—Crystalised chloride of gold and sodium, 1gr. Powder of iris or lycopodium, washed with alcohol to withdraw its soluble parts, 2grs.

Divide the first grain into 20 parts, and increase the dose gradually to one 10th of a grain. Rub one into the tongue or gums, once a day. Four grains thus divided suffice to cure primary venereal symptoms.

GRENADIA,

And the *bark of the pomegranate root*, expel tape-worm.

Take first an ounce and a half of castor oil, and an equal quantity of syrup of lemons; then administer the *decoction of pomegranate.* Water, 2 pints; bruised bark of the root of the *punica granatum*, 2oz.; macerate in the cold for twenty-four hours; then boil by a slow fire until it is reduced to one pint; divide in three doses, one of which is to be taken every half or three quarters of an hour. The worm generally comes away an hour or two after the last dose.

FATTY PRINCIPLE

Of the buds of the male fern (aspidium filix mas). The buds are digested in ether; the fatty matter obtained is administered in pills; one drop to a dose, against tænia. From 8 to 30 drops destroy the worm. (Dr. Peschier.) Most effectual in expelling tænia. (Magendie.)

PHOSPHORUS

Has been recommended in several affections; it appears to be aphrodisiac.

Phosphorus, 1oz. Olive, or sweet almond oil, ½pt.

Cut the phosphorus in very small pieces; introduce them into a well-stopped bottle; let it stand for a fortnight; decant, and scent it with oil of bergamot. It must always be kept in a dark place. 25 or 30 drops may be given daily, for three or four days.

DIGITALINE.

The active principle of digitalis. (Leroyer, of Geneva.)

SALICINE

Is a powerful febrifuge. Dose: 12grs. daily. (Magendie.) It is febrifuge in a small degree. (Andral.)

LACTIC ACID

Is recommended by M. Magendie in dyspepsia, or simple debility of the digestive organs; given in lemonade or lozenges.

Substances recently employed, and not in M. Magendie's Formulary.

MURIATE OF BARYTA,

In large doses, given at La Pitié, in white swellings, by Lisfranc, produced a notable amelioration in the greater number of cases. Several patients take 40grs. daily; M. Pirondi has carried the dose to 2 drams. The muriate of baryta lowers the pulse, which in one patient fell to 25 beats in the minute; it afterwards rose to between 35 and 40. On the average, the pulse is brought down to 50 or 55 beats.[1]

ACONITINE.

M. Hesse, by treating the dried leaves of the *aconitum napellus*, according to the process employed in procuring *atropine*, obtained this alkaline, white, uncrystalisable substance.

It is inodorous; its taste is bitter, then acrid; but the acrid property does not belong to the aconitine, for it disappears when this alkali has been combined several times with acids, and the salts formed have been successively decomposed.

Aconitine, when pure and deprived of its acrid principle, is in the highest degree poisonous. The one 60th of a grain in a little diluted alcohol is sufficient to kill a sparrow in a few minutes; the 15th of a grain kills a small bird with the rapidity of lightning; applied to the eye, it dilates the pupil for a short time only. (For its medicinal effects, see Turnbull.)

It is slightly soluble in water, very soluble in alcohol or ether, and soluble in nitric acid. It completely neutralises acids, but the salts do not crystalise.

COLCHICINE.

Pelletier and Caventou discovered *veratrine* in the bulbs of the colchicum autumnale; a substance also existing in veratrum album and sabadilla. Hesse and Geiger[2] have obtained colchicine by acting on the seeds of the colchicum with boiling alcohol, which takes up an acid salt; this salt is precipitated with magnesia, and the precipitate is again taken up with boiling alcohol; which, by evaporation, allows the colchicine to subside.

[1] Gazette Médicale, July 11, 1835.
[2] Annales de Pharmacie, vol. vii. c. 5, p. 275.

It crystalises in the form of fine needles; it is not acrid, and does not provoke sneezing like veratrine. It neutralises the acids, forms crystalisable salts, and is soluble in water.

Colchicine is poisonous; one 12th of a grain, given to a cat two months old, produced salivation, frequent stools, vomiting, tottering, plaintive cries, convulsions, and death in about twelve hours. The stomach and intestinal canal were violently inflamed, and there was effusion of blood throughout their entire extent.

LOBELINE.

The active principle, which Colhoun (American Ph.) has obtained from the lobelia inflata by acting with diluted muriatic acid, seven or eight hours, on the leaves of this plant, decanting the liquid, and evaporating till crystals formed. To separate the crystals from foreign matter, alcohol is employed, which, on evaporating, leaves this substance in the form of a transparent extract.

Like the lobelia, it irritates the fauces in a peculiar manner. Alcohol dissolves it perfectly; ether, scarcely at all. It forms salts with the acids. The tartrate is deliquescent, and forms indistinct crystals, which are obtained by exposing the salt to a moderate heat, in order to evaporate the water. The sulphate and muriate present the same appearances as the tartrate, and are deliquescent.

The *lobelia inflata* is indigenous in Virginia and Canada, and is cultivated in the gardens of Paris and in this country. The Indians employ it as an energetic emetic.

Barton, Bigelow, and Chapman, state that this plant is acrid and dangerous; producing vomiting, copious perspiration, and sometimes colics and narcotism. By its employment, Cutler cured an *asthma* with which he was afflicted, and wrote a memoir on the subject. Andrews administered the lobelia in asthma, whooping-cough, and spasmodic croup. M. Bidaut de Villiers gave the leaves and seeds in powder or tincture, as an emetic. As an emetic, the dose of the powdered leaves is from ten to twenty grains; half this dose is expectorant. The dose of the tincture is from thirty to forty drops: in New England the infusion is prescribed in leucorrhœa. Dr. Elliotson has employed the lobelia very extensively.

The lobeline obtained by Colhoun is probably impure.

SAPONINE

Is probably an acid combined with potash.

VALERIANIC ACID,

Analysed by M. Effling:—10 carbon, 18 hydrogen, 3 oxygen.

It is obtained from the essential oil of valerian.

BIGNONIA CATALPA

Was given by M. Antomarchi, with success, in asthmatic affections.

A decoction made by boiling the seeds and diaphragms of three or four pods in 12 ounces of water, till reduced to 6 ounces, was

given night and morning. Kaempfer and Tauberg employed the bignonia in similar affections.[1]

BALLOTA LANATA.

Recommended in rheumatic, arthritic, and gouty affections, by Professor V. L. Brera.

This plant is indigenous in Siberia, where, according to Pallas and Gmelin, it is employed in dropsy. "I had," says the professor, "an opportunity of verifying, in my practice, the efficacy of the diuretic and resolvent properties of this plant in dropsies depending on visceral congestion. And as, in employing it, I found that it succeeded more frequently in dropsies connected with rheumatism and arthritic diseases, I prescribed it directly in these affections. The success surpassed my expectations." Several facts in support of this favourable opinion are cited.

Dose: ʒ ss to ʒ i were given in decoction (ʒ viii) daily.

CREOSOTE.

Reichenbach, of Blansko, in Moravia, discovered in tar the creosote, eupione, paraffine, picamar, pittacal, and capnomor.

The creosote he prepares from tar of the beech tree, by six distillations, and by dissolving it afterwards in a solution of caustic potass three times, setting it free successively with sulphuric acid.

It may be obtained, of equal purity, from the tar of the pine tree, and with fewer operations.

The creosote should be colourless, and have a specific gravity of 1.037 at 20° cent. Its alcoholic solution, mixed with baryta water, should not turn brown on exposure to the air. It boils at 203° cent., and distils over without alteration. Its odour is peculiar, approaching that of smoked meat and castoreum. In its general properties it resembles the essential oils. Several medical men in Germany have confirmed its anti-putrid, desiccative, and styptic properties. It has been applied in flabby, carcinomatous, and syphilitic ulcers, in *dartre*, scabies, and even caries, with signal success.

It is generally used externally, either pure, or in a solution prepared by distilling one part of creosote with eighty of water. To obtain a more concentrated solution, dissolve ʒ i of creosote in ʒ ii of alcohol, which then drop into water till it begins to become opaque. The creosote is an excellent application to aching, carious teeth.

Reichenbach relates many facts tending to prove its efficacy in dysentery, and believes the inhalation of creosote would be highly advantageous in tracheal or bronchial suppurations. Experiments made in Munich on dogs prove that its action is topical. One of the carotids was divided, and the bleeding was checked for some moments by holding the vessel between the fingers, while a pad moistened with creosote was applied. When the artery was let

[1] Journal de Chimie Médicale, March, 1834.

loose, the blood did not flow, and the wound healed in a few days without a bandage.

The aqua binelli, an Italian quack medicine, celebrated at Naples for arresting hemorrhage, was sent to Berlin in 1831. Graefe, who had recently been in Italy, hearing a high report of its efficacy, mentioned it favourably in a clinical report,[1] and proposed trying its effects on animals and in practice. In the report of 1832, he speaks of it in more moderate terms; he had tried it in amputation of the breast and arm, but was obliged to resort to the ligature afterwards. Berzelius failed in ascertaining the nature of this water, which remained unknown till Schweiger Seidel stated as his belief that it was a solution of creosote lately discovered by Reichenbach. Creosote[2] was then speedily tried all over Germany in those cases which make the despair of surgery, and performed more than a homœopathic proportion of cures, which, as is usual, sufficed to carry its fame to Paris and London, where it is now very generally prescribed. It is extensively applied to burns and foul ulcerations of every kind, to cancerous and syphilitic ulcerations, to carious teeth, *capillary hemorrhages*, and several furfuraceous eruptions; in diabetes, atonic rheumatism, phthisis, and chronic bronchitis, it is said to be beneficial. Dr. Elliotson alone has made a series of experiments on its action in one particular complaint: several cases of vomiting, treated by Dr. Elliotson, and published by Mr. Taylor in the *Lancet*, recovered after its administration. At the Medico-Chirurgical Society, Dr. Elliotson has also stated that creosote is given with advantage in diabetes.

The creosote is generally applied pure; but in 80 parts of water it forms an efficient *solution*, in some cases. It is applied on *cotton* to bleeding wounds. *Creosote ointment* is made with 12 drops of creosote and an ounce of prepared lard. Internally administered, the dose is one drop increased to 10 drops in camphor mixture, or in pills made with an absorbent powder.

ORGANIC ALKALIS.

Geiger divides the organic alkalis into volatile and fixed; but this classification is only relative. Volatile organic alkalis volatilise at the ordinary temperature; in their state of greatest purity they are liquid, and are always distinguished by a peculiar odour. Those not volatile at the ordinary temperature are solid, not alterable in the air; and, when they are very pure, inodorous. But some, obtained from the solaneæ, readily contract a narcotic, nauseous odour. *Hyosciamine* forms, as it were, the transition between the organic volatile and fixed alkalis.

Considered physiologically, they may be divided into the noxious and the innoxious. The volatile organic alkalis are all, as far as we can judge, acrid and poisonous. Among the fixed, *atropine*,

[1] Bericht über des König. Clin. Chirurg. Augenarzt. Institut. der Universitat. zu Berlin, 1831.

[2] Graefe und Walther's Journal, B. 17, p. 652.

hyosciamine, *daturine*, *colchicine*, *aconitine*, also *delphinine* and *emetine*, are acrid or bitter, and poisonous. *Morphine*, *strychnine*, and *brucine* are bitter, narcotic, and poisonous. Quinine and cinchonine belong to the class of organic fixed alkalis, which are bitter and not poisonous.

It is worthy of remark, that all the organic bases contain nitrogen; and that all which have been hitherto examined verify the remarkable law discovered by Liebig, that the proportion of nitrogen gives the measure of their saturating power. An atom of an organic alkali contains exactly an atom of nitrogen.

All organic, crystalisable substances, poisonous or mild, yet active, and similar to organic, saline bases, must not be confounded with this class of bodies—to which a great number of substances, many of them without nitrogen, have been improperly referred. They present, in their physiological effects, many resemblances with the organic bases; thus there is picrotoxine (distinctly poisonous), columbine, and lactucarium, not yet analysed, which come near strychnine, brucine, and the narcotic principles of opium. Elaterine, a drastic purgative, colocynthine, bryonine, and digitaline, present, in their effects, many analogies with veratrine, colchicine, and emetine. Salicine, gentianine, and other crystalisable, bitter principles without nitrogen, have an effect similar to that of quinine and cinchonine.

But as alimentary substances containing nitrogen are more nutritive than those without this principle, so the organic bases are much more energetic than the neutral substances having no nitrogen.[1]

[1] Journal de Pharmacie, Mars, 1834; from Annalen der Pharmacie, 1833, vol. vii. p. 270.

CASES AND OBSERVATIONS,

ILLUSTRATIVE OF

RENAL DISEASE

ACCOMPANIED WITH

THE SECRETION OF ALBUMINOUS URINE.

BY DR. BRIGHT,[1]
Physician to Guy's Hospital, &c. &c.

The importance and extensive prevalence of that form of disease, which, after it has continued for some time, is attended by the peculiar changes in the structure of the kidney, now pretty generally known by the names of "mottling," "white degeneration," "contraction," or "granulation," impresses itself every year more and more deeply on my mind; and whether I turn to the wards of the hospital, or reflect on the experience of private practice, I find, on every side, such examples of its fatal progress and unrelenting ravages as induce me to consider it amongst the most frequent, as well as the most certain, causes of death in some classes of the community, while it is of common occurrence in all; and I believe I speak within bounds, when I state that not less than five hundred die of it annually in London alone. It is, indeed, an humiliating confession, that, although much attention has been directed to this disease for nearly ten years, and during that time there has probably been no period in which at least twenty cases might have been pointed out in each of the large hospitals of the metropolis—and there is reason to believe that double that number may, at this moment, and at all times, be found in the wards of Guy's Hospital—yet little or nothing has been done towards devising a method of permanent relief when the disease has been confirmed; and no fixed plan has been laid down as affording a tolerable certainty of cure in the more recent cases. I believe that our want of success in what are considered the more recent attacks, is frequently owing to the fact, that the disease is far more advanced than we suspect, when it first becomes the object of our attention: and I am most anxious, in the present communication, to impress upon the members of our profession the insidious nature of this malady, that they may be led to watch its first approaches with all the solicitude which they would feel on

[1] From Guy's Hospital Reports, vol. i. p. 338.

discovering the first suspicious symptoms of phthisis or of epilepsy. There is great reason to suppose that the seeds of this disease are often sown at an early period; and that intervals of apparent health produce a false security in the patient, his friends, and his medical attendants, even where apprehension has been early excited.

The first indication of the tendency to this disease is often hæmaturia, of a more or less decided character: this may originate from various causes, and yet may give evidence of the same tendency: scarlatina has apparently laid the foundation for the future mischief: exertion in childish plays has done the same; or it has sometimes appeared to be connected with suppressed catamenia. Intemperance seems its most usual source; and exposure to cold the most common cause of its development and aggravation. It is, however, more particularly to those causes which operate in youth, or are apparently so casual as to tempt us to believe that when the immediate symptoms are subdued no evil can result, that I wish to direct attention. Where intemperance has laid the foundation, the mischief will generally be so deeply rooted before the discovery is made, that, even could we remove the exciting cause, little could be hoped from remedies; but, at the same time, a more impressive warning against the intemperate use of ardent spirits cannot be derived from any other form of disease with which we are acquainted; since, most assuredly, by no other do so many individuals fall victims to this vice.

The history of this disease, and its symptoms, is nearly as follows:—

A child, or an adult, is affected with scarlatina or some other acute disease; or has indulged in the intemperate use of ardent spirits for a series of months or years: he is exposed to some casual cause or habitual source of suppressed perspiration: he finds the secretion of his urine greatly increased, or he discovers that it is tinged with blood; or, without having made any such observation, he awakes in the morning with his face swollen, or his ankles puffy, or his hands œdematous. If he happen in this condition to fall under the care of a practitioner who suspects the nature of his disease, it is found that already his urine contains a notable quantity of albumen: his pulse is full and hard, his skin dry, he often has headache, and sometimes a sense of weight or pain across the loins. Under treatment more or less active, or sometimes without any treatment, the more obvious and distressing of these symptoms disappear; the swelling, whether casual or constant, is no longer observed; the urine ceases to evince any admixture of red particles; and, according to the degree of importance which has been attached to these symptoms, they are gradually lost sight of, or are absolutely forgotten. Nevertheless, from time to time the countenance becomes bloated; the skin is dry; headaches occur with unusual frequency; or the calls to micturition disturb the night's repose. After a time the healthy colour of the countenance fades; a sense of weakness or pain in the loins increases; headaches, often accompanied by vomiting, add greatly

to the general want of comfort; and a sense of lassitude, of weariness, and of depression, gradually steal over the bodily and mental frame. Again the assistance of medicine is sought. If the nature of the disease is suspected, the urine is carefully tested; and found, in almost every trial, to contain albumen, while the quantity of urea is gradually diminishing. If, in the attempt to give relief to the oppression of the system, blood is drawn, it is often buffed, or the serum is milky and opaque; and nice analysis will frequently detect a great deficiency of albumen, and sometimes manifest indications of the presence of urea. If the disease is not suspected, the liver, the stomach, or the brain divide the care of the practitioner, sometimes drawing him away entirely from the more important seat of disease. The swelling increases and decreases; the mind grows cheerful or is sad; the secretions of the kidney or the skin are augmented or diminished, sometimes in alternate ratio, sometimes without apparent relation. Again the patient is restored to tolerable health; again he enters on his active duties: or he is perhaps less fortunate;—the swelling increases, the urine becomes scanty, the powers of life seem to yield, the lungs become œdematous, and, in a state of asphyxia or coma, he sinks into the grave; or a sudden effusion of serum into the glottis closes the passages of the air, and brings on a more sudden dissolution. Should he, however, have resumed the avocations of life, he is usually subject to constant recurrence of his symptoms; or again, almost dismissing the recollection of his ailment, he is suddenly seized with an acute attack of pericarditis, or with a still more acute attack of peritonitis, which, without any renewed warning, deprives him in eight and forty hours of his life. Should he escape this danger likewise, other perils await him; his headaches have been observed to become more frequent; his stomach more deranged; his vision indistinct; his hearing depraved: he is suddenly seized with a convulsive fit, and becomes blind. He struggles through the attack; but again and again it returns; and before a day or a week has elapsed, worn out by convulsions, or overwhelmed by coma, the painful history of his disease is closed.

Of the appearance presented after death, enough will be said in another part of the present communication: but one question may be asked in this place—Do we always find such lesion of the kidney as to bear us out in the belief that the peculiar condition of the urine, to which I have already referred, shows that the disease, call it what we may, is connected necessarily and essentially with the derangement of that organ? After ten years' attentive—though perhaps I must not say completely impartial—observation, I am ready to answer this question in the affirmative; and yet I confess that I have occasionally met with anomalies which have been somewhat difficult to explain.

I have certainly seen one or two cases, and have read statements of one or two more, in which the condition of the kidney would have led me to expect albuminous urine, but in which it had not been found to exist. In all these cases, however, the observations

on the character of the urine have been made only a few days or weeks before death, at the close of a protracted illness; or the disease of the kidney has been complicated with other very extensive disease. A case occurred under my care in the Clinical ward this winter, where a man died with ascites and a complication of most extensive disease of the liver and peritoneum, with moderately advanced granulation of the kidney; yet it was only by the most careful examination that any traces of albumen could be detected in his urine: and this leads me to observe, that the secretion is apt to undergo changes, even after the structural disease is established; which renders it requisite that we should not be content with examining the urine on one or two occasions, if we have any reason to suspect the existence of this disease. In the first place, it is quite certain that if, from any cause, the urine becomes alkaline, the application of heat generally fails to produce coagulation; and in the next place, there has appeared to me to be an occasional alternation in the secreting power of the kidney; so that a large quantity of the lithates, or of crystalised lithic sand, is deposited, and the albuminous matter is not thrown off. I have this winter had a case of this kind likewise under my care, in a man whose symptoms bear all the character of renal disease, complicated with the disease of other viscera. His urine for several weeks was found to be distinctly albuminous: it then became loaded with the lithates; and now throws down abundant crystals of lithic sand, and no longer affords any trace of albumen: and mentioning this case to Dr. Addison, I was told that very lately the converse of this had shown itself in a case to which he had been called. All the symptoms led him to suspect this peculiar form of renal disease; but the urine did not coagulate, and was loaded with lithates. After a short time, the lithates disappeared, and now the albumen is very decidedly perceived in the urine. That such facts as these tend, in some degree, to render the presence of albumen in the urine, or its absence, a less unerring test, cannot be doubted; but these anomalies are so few as to interfere very little with the general fact: and after all, in the present state of our knowledge, how few of our diagnostic marks are not more or less under the influence of the casual complications of disease! There is do doubt, likewise, that the morbid condition of the kidneys connected with this disease varies, in different cases, to such a degree as to lead to the belief that the action from which the change has resulted must at least be modified by circumstances and constitutions. The kidney is sometimes simply contracted and hardened; sometimes loaded with an adventitious deposit; sometimes apparently degenerated throughout its whole texture; sometimes affected both with deposit, degeneration, and contraction; all probably the result of chronic excess of action. It is to be expected that modifications should arise in the degree and constancy of the morbid secretion, under such varieties of diseased appearance: but this is not, as yet, satisfactorily known; and I have certainly not always found the quantity

of albumen increased in proportion to the apparent advance in the structural disease.

Another very important question is, the length of time which this disease may exist in the constitution before it runs to its last fatal period: and although our experience in the hospital is great, the point of duration is yet undetermined; for, with all the advantages which an hospital affords for the multiplied accumulation of facts, there are some points on which the information derived in its wards is defective, and even apt to be erroneous; and amongst these may be reckoned one of great importance—the probable duration of life under any disease. If a case is much relieved, the hospital physician loses sight of it, and in all probability sees it no more; knowing nothing of future relapses, or of the ultimate result. On the other hand, a very large proportion of his cases are arrived at the most advanced stages of the respective disorders: the circumstances of the patients have been such as to render them inattentive to the earlier indications of disease; and it is only when they can no longer pursue their laborious occupations that they are driven, too late, to seek relief. Hence the physician is liable to form a wrong estimate of the progress of the disease under more favourable circumstances; and it is necessary to correct his views by a comparison with the history and results of private practice.

There has not yet, perhaps, been sufficient time since this disease of the kidneys first attracted attention, to say to what extent life may be prolonged while the body is under its influence; but, I believe, with care its fatal effects may be kept at bay, and a hazardous life may be protracted for many years. Should that care be neglected, the chance of life will be greatly diminished.

The cases which I now offer will be found to bear upon many points in the history I have just sketched out; and, amongst others, will tend to illustrate the subject of the probable duration of the disease, and some of the more insidious attacks which attend the fatal termination.

CASE I.

Albuminous urine—Death from apoplexy, after the disease had existed at least four years.

In the month of March, 1832, a physician, aged about 42, who had always lived freely, but not intemperately, applied to me, labouring under all the marked symptoms attendant upon the secretion of albuminous urine; his flesh wasting; his strength failing; his legs swollen. It appeared, that for nearly two years these and other symptoms had existed, and he had been treated by different medical men as labouring under diabetes, and under disease of the heart, and various other disorders. The urine was albuminous in the higest degree. I put him on the use of the decoction of the pyrola umbellata; and, after ten days, I added to this two grains of salicine, three times a day; and ordered him to employ a milk diet. He took milk and water for breakfast; a good ordinary dinner, with two glasses of port wine and water; and milk and

water again for tea. After he had continued this treatment for about six weeks, under the watchful care of his friend, Mr. Chapman of Tooting, I received a letter from him on the 28th of April, informing me of his intention to go to North America; where he had formerly spent many years, and where he thought he enjoyed better health than in England. To this letter he added :—"I am happy to say that your prescription has produced a favourable change; and, by persevering in the same treatment and diet, I may perhaps recover completely. My legs have gone down, and the thirst has diminished; and altogether I find myself better. The urine still contains a great quantity of albumen, but the secretion is not so great. The only unfavourable symptoms are want of sleep; and no increase of substance, but rather the contrary, for I think I am thinner than I was: however, I live in hope.

October 24, 1833: he entered my study, looking robust and strong, and in apparent health. He told me that he had spent above a year in Canada and North America, and had returned to England about three months ago: that from the time of his leaving England he had found his health improve; and when he returned he appeared to be quite well. However, some of his symptoms had again shown themselves since his return: his legs began to swell, so that he was obliged always to wear bandages: his hands also swelled occasionally: he had great inability to sleep at night: suffered from vertigo: his urine was coagulable by heat, though it emitted a urinous smell: appetite good: tongue clean: pulse from eighty to ninety: skin freely perspirable. He kept his bowels regular by pills. He remained in tolerable health; but was always excessively low-spirited, and suffered much from headache: he had constantly a slight tendency to anasarca, and his urine was always albuminous.

In the latter part of the summer of 1834 he again resolved, at all hazards, to try the effect of revisiting America; and he seemed, to a certain degree, recruited by the voyage. But in the latter end of November, having exposed himself a good deal in shooting, he was attacked with hemiplegia; and in three weeks after died apoplectic, on the 15th of December.

In this case, it does not appear what was the precise cause to which the disorder of the urinary secretion owed its origin. Its existence is plainly traced through a period exceeding four years; during which time the patient seems never to have enjoyed any thing approaching to a perfect state of health, though there were times when the symptoms had ceased to be recognised by ordinary observers. Mild remedies, and diet, certainly did him temporary good; but the faulty secretion of the kidneys remained, and very little inattention was sufficient to bring on that crisis in which he died. It is observable, that the skin was generally in a perspirable state; and perhaps to this was owing the protracted character of the disease.

CASE II.

Albuminous urine—Death with convulsion and coma, after the disease had probably existed eight years—Kidneys granulated.

In the month of July, 1835, a medical gentleman from the country, aged 33, came to consult me with regard to his general health. His countenance was somewhat pallid; otherwise his looks did not bespeak material disease. He told me that his health had been good till about the age of twenty; when he began to complain of occasional dyspepsia, from which he had continued to suffer, and which had latterly increased. In the year 1826 he had the ulcerated sore-throat of scarlatina, but no rash; and seemed to have taken the complaint from attendance on patients affected with it. He was at that time very seriously ill; and his urine was observed to be deep-coloured like coffee. This however soon ceased; but the urine had generally been of a light colour, abundant, and often inclined to froth, and, he believed, had always been albuminous since that time. It had nevertheless on two occasions, first in 1833, and next in February 1835, put on the same dark appearance as in the year 1826.

Six years ago he had begun to suffer from occasional pain in the back and region of the kidneys, rather more on the left than the right side; and he had wandering pains about the heart, which were considered to be neuralgic. The latter symptoms subsided, but the dull pain in the back continued; and he had since been subject to puffy œdematous swelling of the face, legs, and ankles. Four years ago he had an attack of catarrhal inflammation of the chest, which appeared to have rendered him very susceptible of atmospheric changes; and about a year and a half ago he wasted considerably. He had suffered for the last fifteen years from severe attacks of sick headache, with vomiting, which continued for several hours; and this had increased during the last few months. The fluid which was ejected from his stomach was excessively acid, and sometimes tinged with bile: the bowels were habitually costive. I understood that he has been temperate as regards drink, but irregular in his meals. I examined his urine carefully, found it of a light straw colour, very acid, and throwing down numerous flocculi of albumen by the application of a moderate degree of heat. He evidently saw that I considered his case one of a most serious character; and he consented to do any thing I would direct, consistently with his continuing to practise his profession. I enjoined upon him the greatest care in avoiding exposure to cold and chills; told him to clothe himself completely in flannel; to keep strictly to a milk diet; and to regulate his bowels with scammony or some simple purgative; and I prescribed powders of the uva ursi, soda, and compound ipecacuanha powder, to be taken thrice a day.

I saw him on the 1st day of October following, improved in appearance, and, as he said, greatly improved in his power of exertion and in his general feelings; but the urine still coagulable. He

ascribed this favourable alteration entirely to a most rigid adherence to the milk diet, and the occasional use of scammony to regulate the bowels. He had not continued the powders long.

The next report I obtained of this gentleman was an account of his death, which had taken place on the 27th of November, about eight weeks after I had last seen him apparently so much improved. I learned that about six weeks before his death he had suffered most severely from headache and vomiting; which was now accompanied by dimness of sight, or a kind of mistiness as he described it; and a whizzing in his ears, which was not more on one side than on the other. During the last month, while he was still going about, he complained of an inability to direct his arms and legs, which affected the left rather than the right side. For the last fortnight he kept his bed; and during that time he had frequent twitchings of different muscles; and generally at night, when he fell asleep, was attacked with a convulsion which almost raised him from his bed. On the fifth day before his death he had three attacks of epilepsy, and one or two on the following days: during the last two days of his life he lay in a comatose state. The body was very carefully examined forty-five hours after death; and Mr. Hilton, who conducted it, has favoured me with the following minute particulars of the appearances.

Sectio Cadaveris.—Putrefaction had advanced rather rapidly: the body gave off that very unpleasant smell which I have experienced in cases of a similar nature; and had the cadaveric sanguineous cellular infiltration on the posterior part of the thorax and abdomen, and the course of the cutaneous veins marked by the putrefactive exudation of blood through their tunics into the surrounding tissue, which was yet fluid and homogeneous in its appearance. Some frothy mucus, slightly discoloured from decomposition, had exuded from the mouth. The eyes were but little shrunk, clear, and the pupils open, or rather dilated: no œdema of the legs or arms: limbs firm, fixed, and muscular: fingers strongly fixed, and feet extended.

Abdomen.—The great omentum was drawn up to the left side, and almost concealed at the cardial extremity of the stomach near the spleen: it was not, however, contracted or corrugated, as from the effect of inflammation, but easily unfolded: was transparent, and contained no fat. The other viscera were in the natural position; mesenteric glands being slightly enlarged, but presenting no appearance of suppuration or softening. Peritoneum of the abdominal parietes thickened, without appearances of recent inflammation. The bladder contained about half a pint of clear, light, straw-coloured fluid, not coagulable by heat. About three ounces of slightly turbid reddish serum among the pelvic viscera in the lumbar region. The liver appeared healthy; the granular structure was, however, a little more distinct than natural. On making sections of it, the blood, being fluid, flowed freely. The inferior surface of the liver, in contact with the stomach, was of a dark blue

colour, from gaseous decomposition. Gall-bladder contained about an ounce of well-coloured bile, which, on pressure, passed into the duodenum. The pancreas was healthy, and not at all enlarged. Considerable pressure on this gland caused some of its fluid, which was light-coloured and slightly opaque, to appear at the duodenal opening of the duct. Some old peritoneal adhesions were found at the posterior surface of the stomach, just above the pancreas. Œsophagus healthy throughout. The stomach was of ordinary capacity, not thickened: the rugæ of its interior having chiefly a longitudinal direction, little curved, and most conspicuous towards its pyloric extremity: its submucous tissue was rather more vascular than natural, from congested veins: pylorus perfectly healthy. In the first two inches of the duodenum, the mucous membrane was abraded; surface rough, uneven, and granular, from enlarged solitary mucous glands: the whole rather minutely injected with blood-vessels. This diseased condition extended to within a quarter of an inch, or less, of the pyloric opening, where the line of distinction from the healthy points was well marked. In the opposite direction it did not extend quite so far as the ductus communis choledochus; but terminated in a very irregular edge, as if ulceration had been advancing towards that part unequally. The other portions of the small intestines were healthy; excepting at the lower part of the ileum, where several patches, varying in extent from three to four inches, presented the edges of the valvulæ conniventes a good deal injected. There was some bile in the small intestines, mixed with mucus and a pultaceous mass of nutritious ingesta. The sigmoid flexure of the colon appeared to have suffered from irritation of long duration, manifesting more general arterial vascularity than natural; and in some parts, to the extent of a shilling or half-crown, the mucous membrane was minutely injected, and slightly raised, with distinct and enlarged mucous glands, and small vessels passing to each. Some of these spots were near the termination of the colon. Spleen, healthy and firm. The kidneys presented the minutely mottled degeneration, in their secreting structure, depicted and described in Vol. I. of Dr. Bright's Medical Reports, in connection with coagulable urine. The right was rather more advanced in this change than the left; but neither was softened, nor much increased in size. The ureters, bladder, and urethra, were healthy.

Thorax.—About three drams of slightly-coloured fluid, coagulable by heat, were found in the pericardium. No pleuritic adhesions were observed, except at the upper and outer part of the right lung. The left lung was emphysematous, presenting vesicles on the external surface; one as large as a chestnut occurring on the convex part, and several smaller ones at the lower edge. Structure of the lungs healthy, and not at all tuberculated; the posterior part, however, slightly œdematous, from cadaveric infiltration. Each pleural cavity contained six or eight ounces of serous fluid, tinged with blood; probably imbibed from surrounding parts after death, as there were no obvious symptoms of recent pleurisy.

Heart, large and flabby. The right side contained a good deal of thick fluid blood, and some coagula, with the colouring particles uniformly diffused, showing great deficiency in the vital power of the blood: these were adherent, by being entangled in the musculi pectinati of the right auricle and ventricle—more especially the latter. Valvular apparatus, on both sides, healthy; with the exception of a little thickening of one of the aortic valves, at its attached edge. The wall of the left ventricle was hypertrophied, but flaccid; the fibres remarkably pale, almost white, and very soft, easily breaking down on pressure. The cavity of the left ventricle was perhaps a little too large, in proportion to the other cavities of the organ. Lining membrane of the aorta red, and highly tinged by infiltrated blood, which could not be removed by scraping, but did not extend to the middle coat. No coagula in the aorta.

Head.—The superior portion of the frontal bone was much less prominent on the left than the right side: this had been observed some time before death. The bone at this part was not thickened, but the dura mater adhered more firmly than elsewhere; and, when exposed, was found loose and flaccid on the brain, corresponding to the external depression of the cranium. No coagula in the superior longitudinal sinus. The veins contained some globules of air, which had passed from the thorax through the internal jugular during the inspection of that cavity. On raising the dura mater carefully, there was a layer of coagulated blood recently effused, about the size of a half-crown, resting between the two layers of the arachnoid membrane; not firmly adherent to either, but more so to the arachnoid of the dura mater, corresponding to the external cranial depression where that membrane was flaccid. Arachnoid opaque and thickened, (looking like wet, thin, white leather,) from chronic inflammation over nearly the whole of the vertex on each side; rather more on the left, with some little serous effusion under the arachnoid. A small deposit of bone upon, or rather growth of bone from, the arachnoid, on the pia mater, somewhat larger than a pea, was observed under the loose portion of the dura mater, presenting a superior nodulated, uneven surface. There were also several cartilaginous spots, but none larger than the section of a pea, all on the left hemisphere of the brain. A portion of the falx major, near its junction with the tentorium, was ossified to about the extent of a sixpence, with irregular edges. The pia mater was not adherent, except over the superior part of the left anterior cerebral lobe, where it was slightly so, and some patches of cineritious structure came off with it; and, at this point, the convolutions were not so prominent as at the corresponding part on the right side. The cineritious matter remarkably pale and exsanguine; its two layers easily separable by a little pressure between thumb and finger: this was more especially observable on the left side. The cranial portion of the carotid and vertebral, as well as the basilar arteries, quite healthy and devoid of coagula. No fluid was found in the lateral ventricles; nor was there any morbid appearance, except that the left corpus striatum

was not quite so large or prominent as the right, and that the arachnoid on it was remarkably opaque and thick at its outer edge, throughout its whole length, and for nearly a quarter of an inch in breadth. The outer side of the left thalamus presented, more distinctly than usual, an oval elongated eminence on its superior and external aspect. On slicing the corpus striatum, several cavities presented themselves, having in them branches of arteries quite empty. One of these tubular cavities was sufficiently large to admit a common quill; the others were smaller. It appeared as if the arteries had passed through an oval cyst, or that they had been enlarged to the dimensions of the cavity, and were now contracted. No aneurismal appearance was observed in the artery. The right side of the brain did not present any particular deviation from health.

Vertebral column.—The spinal marrow and its membranes were examined with care, but did not offer any thing pathological. There was but little spinal fluid within the membranes, and nothing like effusion of either serum or blood.

The whole history of this case is most interesting; showing, as it appears to do, the connection of this disease, in the first place, with the scarlatina; presenting traces of it through its lengthened progress, attended by occasional indications in the obvious character of the urine—in the slight and often evanescent œdema of various parts—in the tendency to emaciation—in the returns of vomiting and severe headache—and, at last, in the more decided cerebral symptoms; which, after a succession of convulsive seizures, terminated in coma, when probably eight years, at least, had elapsed from the first commencement of the disease. The examination after death is no less interesting; as throwing light on all the various symptoms, and showing a marked example of the structural change slowly established in the kidney, together with some of those alterations in other organs which so frequently accompany this change as to give us great reason to believe in their being pathologically connected.

CASE III.

Albuminous urine—Death with convulsion, after the disease had probably existed four years.

Dec. 25, 1835, I was requested to come immediately to meet Dr. Prout, in the case of Mr. W——, aged 17. I found him lying on his bed, with his eyelids half closed, and his eyes rolling about in a kind of convulsive motion; his left hand lying powerless across his body, and his left leg almost paralysed. His countenance was pale, tongue furred, and dry in the centre; respiration rather loud; pulse above 100, sharp and wiry; general surface, but particularly the head, warm. He complained of pain in the head, and lifted his hand to his forehead. His hearing was preternaturally acute; his vision nearly perfect, so that he saw and distinguished those around him; and he put out his tongue the instant he was desired.

His urine was pale-coloured, acid, and very coagulable by heat. While we were in the room he had a fit, in which both the leg and arm of the left side were violently convulsed; and he cried out with the pain he suffered, piteously requesting us to lay hold of his convulsed limbs. He seemed perfectly intelligent during the whole fit, and answered questions readily. The paroxysm lasted only a few minutes, after which he became placid, and went into the state of repose in which we first found him.

I learned, on enquiry, that, when only twelve years of age, being at school, he was attacked, after some over exertion, with hæmaturia; and this had recurred several times since, with very copious flow of urine, and a very frequent desire to pass it, so that he was called up three or four times every night. On one occasion, a small calculus had passed, which it is believed was composed of lithic acid, and he had been subject to great headache and constipation of bowels.

About eighteen months ago Dr. Prout saw him, when labouring under an aggravation of his symptoms; and, at the time his attendance was discontinued, the urine was still very coagulable, in which condition it always remained. About three weeks ago he had attended a funeral, and was much exposed to cold; since which he had been obviously worse, though never kept from business. He was seen by Mr. Odling about ten days ago, who found that he had suffered more headache than usual; bowels constipated; tongue brown, and loaded. He was ordered leeches to the temples, took purgatives, and got so much better, that on Saturday, the 19th, he was occupied in some business, which required great exertion both of mind and body, and exposed him to the cold; and he found himself so much confused that he retired to bed; and when he was found there, about nine o'clock, he was affected with a most intense headache, vomiting, and a total loss of sight. Mr. Odling, seeing him in this state, got a large number of leeches; but, whilst he was in the act of applying them to his temples, a severe fit came on, of an apoplectic character, in which the patient's countenance became dark and suffused, and his breathing stertorous. He was bled freely from the arm, and this was repeated three times during the night; but he experienced two more fits, like the first. In the morning Dr. Prout saw him, and ordered cupping from the loins; and, whilst this was being done, a fit occurred. On the Tuesday, fits, assuming more the character of epilepsy, occurred; in which the left side, which had remained weak after the first fits, was convulsed. On Wednesday, the fits returned; but it was evident that the mind was unimpaired during the convulsion. On that night they were frequent, and on the following day not less than twenty occurred. On Friday the 25th, the day on which I was called, they had recurred at intervals of about an hour, and were rapidly increasing. At eight o'clock in the evening he became quite insensible, but he lived, with frequent convulsions, till two o'clock on Saturday morning; thus lingering nearly a week from the first appearance of the severe cerebral disease, during the last five days

of which time it was necessary to draw off his water every eight hours.

No examination was permitted in this case; but it is scarcely to be doubted what would have been the character of the morbid appearances. In this instance the disease had, in all probability, existed for a period of four years, before it proved fatal.

For the particulars of the following case I am indebted to Mr. Weatherfield, and I give it in his own words. The kidneys presented the most perfect specimens of the granular contracted variety of the disease; and Dr. Babington has given me a specimen of the nitrate of urea, obtained by Dr. Barlow from the fluid of the ventricles of the brain.

CASE IV.

Albuminous urine—Death with convulsion—Kidneys granulated.

"About the month of December, 1831, I was consulted by Mr. J. G. He was then suffering under dyspepsia, accompanied with frequent attacks of pyrosis: his complaints, he then told me, had been of long standing. I ordered him a few doses of blue pill, rhubarb, and magnesia, which relieved the most troublesome symptoms; and I did not see him again until August, 1832, when, from a slight injury, fungous excrescences arose on the extremities of two fingers, which for a time resisted all remedies, pouring out a considerable quantity of watery discharge, of a peculiarly faint odour: the application of Goulard water, with pressure, the patient taking at the same time alterative medicines, at length healed these wounds in the September following.

"*Sept.* 1833.—At this time a total change had taken place in all Mr. G.'s symptoms: he had for some time lost the dyspepsia and pyrosis; his appetite was good; and, to use his own words, he could digest any thing; but complained of pain and throbbing in his head, intolerable thirst, huskiness in the throat, swelling of the legs towards night, and of the face in the morning; his pulse was now hard; tongue covered with a creamy coat; urine very abundant. He was advised to be bled, but he neither submitted to that, nor pursued any plan of treatment steadily. He continued much in the same state till February, 1834, when the pain in the head became less frequent, but more severe, returning generally once a week, attended with distressing vomiting; the œdema of the face and extremities somewhat increased: there was now, also, some fluid in the cavity of the abdomen. Soon after this he took a long journey into the country, where he underwent great fatigue, and suffered from a more severe attack of head affection and vomiting than before; which induced him to return to town and consult Dr. Babington, which he did on the 26th of March. After a very careful and patient investigation of the case, Dr. Babington, having tested the urine, and finding it to contain albumen, declared it his opinion that the kidney was the seat of disease. He was ordered to take alterative mercurials and tonics, and to be cupped over the

region of the liver, where he complained of pain, increased upon pressure. The following week he saw Dr. Babington again, when he complained of noise in the left ear, and slight numbness of the right hand and foot; the pulse at this time was feeble. Dr. Babington, however, ordered blood to be taken away, if any aggravation of symptoms should occur. On the morning of the 5th of April he was attacked with epilepsy, and a medical gentleman living near immediately bled him. The pulse rose wonderfully, and continued so strong as to induce us to repeat the bleeding the next day; which, together with purgatives, subdued the convulsions for a time. On the 11th of April, however, he was suddenly seized with another fit, which returned at longer or shorter intervals, until the following day, when he sank.

"Sectio Cadaveris.—The membranes of the brain, particularly the pia mater and arachnoid, much thickened; more than two ounces of water in the ventricles; the brain being altogether firmer than usual; the septum lucidum thickened and tough; plexus choroides paler than usual.

"The lungs were emphysematous; the air-cells much enlarged and ruptured.

"The heart rather larger than usual; the left ventricle somewhat thickened, and the semi-lunar valve slightly ossified; some water was found in the cavity of the chest.

"All the abdominal viscera healthy, except the kidneys, which were much smaller than usual, of a gray and granulated structure."

For the details of the following case, I have to acknowledge the kindness of Dr. Babington and Mr. Wheelwright, under whose care it was, and by the former of whom the history has been drawn up; and its interest is somewhat enhanced, by the fact that the urine had not been examined, and that nothing had occurred which could, under ordinary circumstances, have led to a suspicion of the kidneys being affected: yet the insidious disease had been silently making its way, till it terminated with successive symptoms, so precisely similar to those of the other cases I have related, that the examination after death—which showed that the kidneys alone, of all the organs of the chest and abdomen, were diseased—served at once to explain the otherwise anomalous train of events, and to account for the little benefit derived from remedies which, in the present state of our knowledge, appear to be altogether powerless in this stage of the disorder. This case affords a strong example of the disease of the kidney passing to its most fatal period, without the slightest symptom of dropsical effusion—a state of things, which, above all, is apt to throw us off our guard, and lead us to overlook the derangement of the kidney; and it points very forcibly to the value of the information, which is probably, in many cases, to be acquired by no other means except the careful examination of the urine.

CASE V.

Death, with convulsion and coma—Kidneys granulated.

"Mr. P., an athletic young man of twenty-five years of age, by trade a plumber, came to Mr. Wheelwright on the 6th of December last, complaining of dyspepsia, with some degree of dimness of sight. These symptoms being referred to biliary disorder, some aperient medicine, followed by bitter tonics, was administered; and, in the course of two or three days, Mr. P. returned to his employment. On the 24th, he was again complaining; and a third time, on the 3d of January, when he remained under medical treatment until the 12th; and even then continued so much indisposed that he was advised not to return to business. He did so, however, and continued superintending some plumber's work on the top of one of the public buildings until the 21st. On that day he was much exposed to the weather, and returned home ill, as if from cold. In the middle of the night he was seized with a fit of a convulsive, but somewhat anomalous, character; for he required restraint on the part of his attendants, and yet was not altogether deprived of consciousness. Mr. Wheelwright saw him two or three hours after the commencement of the attack, at five o'clock on the following morning; at which time he was totally blind, had a very powerful and quick pulse, and was suffering from intense pain across the forehead. He was immediately bled copiously, and was sufficiently sensible to hold out his arm for the purpose, and to recognise Mr. Wheelwright. A brisk cathartic was also exhibited. In the course of the forenoon he was again bled, and, the pain continuing, he was cupped in the afternoon. I saw him first on the following day, when there was still considerable arterial power, which was seen, as well as felt, in the branches of the temporal artery; the pain across the forehead, though diminished, was not removed, and the blindness continued. As our patient had already lost upwards of forty ounces of blood, we determined to try the effect of mercury, to the extent of producing mild salivation. On the 25th I visited him again, and found his vision somewhat improved, so that he could distinguish the number of fingers held up before his eyes; the pain of the head was greatly relieved. A topical abstraction of blood, by leeches, was, I think, directed on this occasion; and this was to be followed by the application of blisters to the temples. No signs of salivation yet appeared, and the mercurial plan was continued. I did not see our patient again until the 29th, when I found him complaining greatly of the soreness of his mouth, which had increased so much as to engross all his attention. What particularly claimed our notice, however, was a degree of drowsiness, which seemed gradually increasing, and could not be accounted for by any circumstances in the treatment. The mercury had been discontinued for the last two days; the bowels had acted freely, and the urine had all along appeared of good colour and in sufficient quantity. A chloride of soda gargle, and some mild tonic medicine, were prescribed. The drowsiness, however, gradually increased

from this time, until a state of complete coma was established, which terminated fatally on the following day. On examination of the body, within a few hours after dissolution, a small quantity of fluid, certainly less than half an ounce, was found in the ventricles of the brain; and there seemed to be some softness in the substance of the brain generally, about its basis—not, however, to such an extent as to constitute a degeneration in its structure. The optic nerves seemed quite healthy, throughout their whole tract. All the viscera, as well of the abdomen as of the chest, were perfectly healthy, except the kidneys. The peritoneal and proper tunic of these were thickened and opaque; and, when stripped off, exposed the surface beneath, rough, and of a gray mottled appearance. This diseased structure was seen, on section, to extend throughout the substance of the organ; both kidneys were equally affected. The urine was, I regret to state, not particularly examined in this case; for there had not been the least tendency to dropsy during the progress of the disease, nor any complaint of pain in the loins to indicate its seat."

The report of the following case is copied from the Museum Note-book, where, I believe, it was written by Mr. King.

CASE VI.

Albuminous urine of four or five years' continuance—Death with convulsion and apoplexy—Kidneys degenerated.

"Mary Brooks, aged 24, was admitted, under the care of Dr. Bright, into Charity's Ward, where she had been two or three times before. For four or five years she habitually passed coagulable urine. Her blood had been analysed, both by Dr. Prout and Dr. Babington, and had been found to contain urea. During this time she had been repeatedly and greatly anasarcous; latterly, her head had become affected, and she laboured under fits of an epileptic character. Her complexion was pale, but she appeared well nourished, and less sickly than could have been anticipated from the nature and continuance of her complaint. A few days before her death, she called in the ward to see the sister; whilst there, she was seized with a fit, from which she never recovered, but remained insensible to the time of her death. It had never been remarked that her pulse was irregular.

"Sectio Cadaveris.—The cranium was unusually thick, with a general but slight irregularity on its surface; there was a small exostosis, apparently of much closer texture than the rest of the skull, about the size of the end of the little finger, situated at the anterior part, near the falx; and there were one or two much smaller ones nearly in the same situation; beneath the arachnoid was a copious effusion of serum. The cineritious substance of the brain was somewhat injected. The medullary likewise contained more than the ordinary quantity of blood, occasioning a slight marbling, visible on its incised surface; in other respects, the

substance of the brain was healthy, but the lining membrane of the ventricles was rather thickened; and in each plexus choroides there was a small defined body, nearly of the size and form of a small horse-bean, which was of a light yellowish colour, and of a moderate degree of firmness: each appeared to be enclosed in a cyst, and the material within was somewhat grumous, especially near its centre. There was merely the ordinary quantity of fluid in the ventricles; the pituitary gland was perfectly healthy; the pleuræ were free from adhesions; there was some appearance of inflammation in the left lung, posteriorly. There was, likewise, some little appearance of pulmonary apoplexy in the right lung; and also some appearance of inflammation, but to a less extent than in the left. The pericardium was healthy. The heart was of moderate size, and likewise healthy, with the exception of a little thickening of the mitral valve; the aorta was healthy, but there was a minute white spot beneath its lining membrane, near its origin. The peritoneum was generally healthy, but there were some old and very partial adhesions, especially in the pelvis; one of these was so situated as to render internal strangulation a possible event. The stomach and intestines appeared to be healthy, but the solitary glands in the duodenum were rather large. The liver was healthy, but there was a partial thickening of its tunic on the convex surface, connected with an old peritoneal adhesion. The pancreas was healthy; the kidneys were small, rather granular on their surface, of a very firm and dense texture, and of a light colour. From the far-advanced state of the white deposit described by Dr. Bright, their small size was evidently the result of contraction; the cortical part being greatly reduced in thickness, and the surface lobulated, as in the fœtus. The uterus was healthy, but apparently in a state of menstruation. The ovaries were rather plump, and their tunics generally smooth, having one or two equivocal appearances of cicatrices; a cyst, about the size of a hazel-nut, and of a dark colour, projected from near the end of one of them: on being cut into, one was found to contain extravasated blood, part of which was of a dark venous colour, and apparently but recently coagulated; whilst a smaller portion was of a much firmer consistence, and of a lighter and brownish colour, indicating a previous extravasation; it was evidently the result of derangement in one of the vesicles of De Graaf. There were two or three corpora lutea in one or both ovaries."

This young woman was living at the time I published the second volume of my Reports (1831); and I have there said—"In one very remarkable case, where the albuminous condition of the urine has constantly existed, as far as I know, from frequent experiment for above three years, the quantity of urea in the blood is very considerable; yet the patient, who is a young woman, enjoys very tolerable health, and has a healthful appearance between the severe attacks of anasarca, of which she has been the subject, but has latterly been affected with frequent fits, which assume a decidedly epileptic character. The results of chemical analysis, conducted

by my friend Dr. Babington, were, that the urine did not contain one third of the urea which it does in health, while about one per cent. of albumen supplied its place. The serum of the blood was remarkably light, in consequence of its deficiency in albumen—having a specific gravity of 1021, instead of 1030; and the quantity of albumen in 1000 grains of serum amounting, after careful drying, to only 50 grains; whereas from 80 to 100 parts in 1000 is the usual proportion in healthy serum: and it contained fully as much urea as the urine did, the 1000 grains yielding nearly 15 grains of that substance."

As these observations were made during the life of this patient, the circumstances of the examination after death possess great interest. As far as one case can go, we have the full confirmation of the coincidence of the various symptoms, with the peculiar degeneration of the kidneys, which was anticipated from the long continuance of the disease: we likewise perceive that there is at least no necessary connection, although there are such frequent coincidences, between hypertrophy of the heart and the disease of the kidneys, even where it is attended by the presence of urea in the blood, and has proceeded to the hardening and contraction of the organs. With regard to the question of cerebral symptoms, that is left untouched by this particular case, because there was a small bony exostosis from the skull, which might have been the source of the epileptic seizures; though very probably this source of irritation would have remained innocuous, but for the state of the circulation induced by the constitutional disease.

CASE VII.

Albuminous urine—Sudden death—Kidneys mottled.

Maria H——, aged 21, was admitted, under my care, into Guy's Hospital, Feb. 25, 1835, labouring under most profuse general anasarca, with coagulable urine. We learnt that the catamenia had come on at the age of 14, and continued regular for three or four years, after which she became the subject of chlorosis, with irregular menstruation. It was now just fifteen months since she observed that her legs began to swell, after having over-exerted herself in walking; this, however, only continued a week, and for two months she was free from all anasarca. About that time she caught cold, and the swelling had continued ever since, varying from time to time, both in situation and degree. The urine was exceedingly coagulable; and she was subject to most intense headaches, often attended with severe vomiting.

This young woman remained under my care for many months, varying almost every week in a remarkable degree, and subject to repeated relapses of anasarca, which she almost always ascribed to some slight exposure—generally to the draught of an open window. She was kept on the mildest diet; chiefly milk, and beef-tea, and arrow-root. The remedies employed were principally diaphoretics; amongst which the warm bath, the hip bath, and Dover's powder,

were the most important. From the beginning of May to the beginning of September, I confined her entirely to her bed, and frequently obtained a very perspirable state of skin, sometimes almost in excess. This treatment was often suspended and varied, owing to the severe attacks of vomiting. After a time, the catamenia began to return with tolerable regularity. The flow of urine was often very abundant, amounting to four pints in the twenty-four hours, and varying somewhat in the extent of its albuminous qualities, but never quite free from albumen; and at length, the anasarcous swellings being reported as entirely gone, she became very desirous of returning home. I, however, detained her several days, till her mother had provided her with a flannel dress, to wear constantly close to the body; and then, with many precautions, she was to leave the hospital on the following day, September 29; but that very night she died almost suddenly, having only complained in the evening of a severe pain in the loins.

Sectio Cadaveris, *copied from the Museum Note-book.*—"The only noticeable deviations in the head were slight serous and clear effusions in the pia mater, and a very remarkable softness of brain; it was not fully dissected. No fluid in the ventricles; the vascularity of the brain substance was considerable; larynx not examined; the thymus was as large as an egg, flat, and fleshy; the serous cavities were almost healthy, and reddish.

"The heart was of a full healthy size, muscular, and well contracted. Both ventricles were pretty certainly, in some degree, hypertrophic; the right auricle was fully distended; the right ventricle contained but little; the pulmonary valves acted firmly; the tricuspid also acted well.

"The blood was well coagulated, fibrinous, and firm. The lungs were every where crepitant: the right was of a dark red, as if from excessive injection, with a little fulness of the larger vessels. I could detect little or no emphysema, or other alteration of parenchyma; the left lung was, in addition, slightly œdematous, chiefly towards its upper part. The bronchial tubes were a good deal injected, and contained some fluid and pasty mucus.

"The liver was of a good size, yet seemed loose, as if collapsed; it was darkish, especially inferiorly turgid, and the normal structure indistinct; the secretion, watery and mucous.

"The spleen half as large again as natural, coarse and fleshy.

"The kidneys were somewhat larger in proportion, of a pinkish yellow white colour, yet injected; the cortical columns full and coarse, and the surface slightly speckled with still more opaque white spots, minute and flocculent; the tubular masses a little less pale; the tunics were pretty healthy in appearance; the surfaces of the kidney were even, but marked by slight salient linear interlobular boundaries; the whole substance was firm, and perhaps had begun to contract.

"The bladder contained a good quantity of urine, not very pale. The ovaries were enlarged and round, and darkly injected; their

tunics were thin and scarred; many large fluid cysts were found within. The Fallopian tubes healthy; the uterus was lined with a thin layer of grumous blood, firm and adherent.

"The digestive canal was every where somewhat fleshy and œdematous; its peritoneum opaque, and rather pink. The lining of the stomach was still more injected."

CASE VIII.

Albuminous urine—Death with decided cerebral symptoms, after the disease had probably existed for two years.

Jeremiah Collins, aged 50, an Irishman, who has served seven years in the navy, and for nineteen years been a common labourer, was admitted into the Clinical Ward, Feb. 3, 1836. Although he had experienced casual illness before, he considered himself quite well, till two years ago, when he had occasion to drive a cart during the whole of a very rainy day, and was wet through from morning till night. He drank a great quantity of spirits on that day, but not sufficient to produce intoxication. The next morning he found his abdomen a little swollen; penis and scrotum œdematous; and he suffered pain in the head. He has never since been well, having been frequently laid up; and taking medicine, and working, in the intervals. His mouth, he says, has often been made sore; and he enumerates four separate times, under four different medical men, all of good name and repute, who had put him under mercurial courses, without any marked relief.

The present symptoms are, occasional pain and heaviness of the head, felt principally at night; mouth still under the influence of mercury; cheeks puffed; difficulty of breathing, especially whilst in the horizontal posture; cough, which is most urgent at night; some expectoration; sound of heart unnaturally diffused; palpitation on slightest exertion; abdomen considerably distended, and fluctuation distinct; penis and scrotum, together with both upper and lower extremities, œdematous; pupils dilated, but not insensible to light; vision in both eyes has been imperfect for about eight days, so that he cannot recognise any one at the distance of four or five yards.

Feb. 4.—Does not seem so well as he was yesterday. Complains of a sensation of fulness in the abdomen. Fainted this morning at eight o'clock, after which he perspired freely. Bowels open once—motion costive; tongue covered with a brown fur in the centre, white at the sides; pulse 88, rather sharp, but compressible; urine slightly acid, of pale colour, coagulable, and becoming very much like milk in appearance, on the application of heat.

Applicetur Emplast. Cantharidis Nuchæ. Habeat Pulv. Antimon. gr. v. ter die. Ext. Col. c. Cal. gr. xv. statim.

5. Has slept well, and his bowels have been freely opened. The

swelling of the penis, scrotum, and legs, has nearly subsided: pulse 112, weak. He was put upon the milk diet.

This man rallied a little occasionally, and the œdema was sometimes much diminished; but, upon the whole, he seemed to get weaker, and the antimonial powder was given up for the camphor mixture with gentle diaphoretics, and a small quantity of wine was allowed him.

22. He feels better, and the cough and expectoration are much less. One copious healthy motion; about four pints of urine passed in the twenty-four hours, rather dingy. Skin moist; pulse 80.

At 2 o'clock, fainted, and was afterwards sick; vision becoming more indistinct, and the articulation more difficult. He was again sick about 9 o'clock.

23. Complains of giddiness and of drowsiness, but of no particular pain in any part. Dimness of vision increases; he is now only just able to distinguish a pen held in the hand at the distance of a yard; has cold shiverings frequently. The action of the heart is so much increased by the exertion of raising himself in bed, as to be heard over every part of the chest distinctly. Pulse 80, and soft, while he is lying quiet; bowels not open since yesterday; a large quantity of urine has been voided.

The symptoms, which had been increasing upon him, made rapid advances. In the evening he became comatose, and died the following morning. No examination of his body could be obtained.

CASE IX.

Coagulable urine—Death from peritonitis—Kidneys much diseased.

H—— M———, aged 43, was admitted into the Clinical Ward, December 16, 1835, with general œdema, sallow countenance, and pale lips. He passed a large quantity of urine which was highly coagulable; and he had frequent calls to void it. He had considerable cough and expectoration. The account obtained of him was, that he had been a tailor in the corps of marines for twenty-three years, and had lived a very intemperate life: he had, however, enjoyed good health generally, but about a year ago had acute rheumatism and dropsy; since which time he had been less healthy, suffering frequently from cough, which had greatly increased during the last ten days.

He was cupped at the scrobiculus cordis, and had half a grain of elaterium administered. The report of the following day was, that he had expectorated much muco-purulent matter. He had passed several very watery stools. Urine plentiful; and he had fourteen calls to pass it: it was greatly coagulable, both by heat and nitric acid: of specific gravity, 1020. Pulse 84, weak, but regular. Attention was chiefly paid to the cough, and a local affection of the scalp.

On the 22d, six days after admission, the daily report was—

Bowels open three times: motions of a dirty white colour, and first beginning to get pale two days since. No pain in the loins, but a pain over the region of the liver: no œdema in any part of the body: pulse 88: cough troublesome. The urine remained unaltered in its character; his bowels were freely acted upon by frequent doses of calomel and colocynth: he was cupped over the region of the liver; and he took salines and diaphoretics. On the 27th, there was a slight return of œdema; but this again disappeared; and on the 3d of January it was reported—"Bowels open three times: cough and expectoration almost gone: sores on the head nearly healed."

On the 8th of January considerable œdema returned in his legs and arms; but he chose to leave the house, with a view of returning to his occupation as a tailor; and accordingly went, without any intimation of his intention. When I heard that he was gone, I expressed my regret, and stated my fear of the result; but I was certainly not prepared to expect that within three days we should be requested to come to examine the body.

We learnt, that on leaving the hospital, he walked home beyond the town; had a single pint of porter; and expressed his intention of resuming his employment next day: but in the evening he complained of pain in the head, and most intense and excruciating pain in the abdomen; which his sister-in-law said she could compare to nothing but labour-pains rapidly succeeding each other; and he was at times delirious: he likewise suffered from excessive shivering and coldness, so that he had a large fire made and sat before it, opening the blanket in which he was enveloped to admit the heat. This state of things continued more or less through the whole of the following day and night; and on the next day, about two o'clock, having for two hours before his death become apparently blind, he died; little more than forty-eight hours having then elapsed from the time when he left the hospital.

Sectio Cadaveris.—The arachnoid was slightly opaque, particularly along the middle of the sulci of the convolutions; and there was rather more fluid than natural, both beneath it and in the ventricles. The lungs were healthy and crepitant, without adhesion to the pleuræ: the heart large and muscular, but the valves healthy. The abdomen contained a full pint and a half of serum, evidently the result of acute inflammation: it had nearly the consistence of very thin arrow-root or gum-water, opaque, but not puriform. The liver was quite healthy in its structure; but had an unnaturally light and opaque appearance, owing to a thickened condition of its peritoneum. The kidneys were diseased to the utmost degree: they adhered very strongly to their tunics; were lobulated in shape, large, and almost white, with a slight tinge of yellow. When cut into they were almost cartilaginous in consistence, showing the same perfectly white colour, with specks of yellow throughout the cortical part; the tubuli presenting a very strong contrast, by their deep red colour. Of these kidneys a very

beautiful model was executed in wax by Mr. Town; and is deposited in the museum.

CASE X.

Albuminous urine—Death from peritonitis—Kidneys diseased.

W. Burford, aged 21, was admitted into Guy's Hospital, under my care, September 23, 1835, the subject of general anasarca, with urine dingy in colour, acid, and very coagulable by heat. He complained of some pain and uneasiness in the loins, and his skin was hot and dry. He was put upon the use of antimonials and Dover's powder; was kept strictly in bed; was cupped on the loins; and subsequently had a seton in that part. It soon however appeared that his stomach was peculiarly irritable; and it was necessary to change the remedies, as also to relinquish the milk diet. He made some progress for a time, but it was neither steady nor satisfactory; and in the second week of January he was seized with acute peritonitis, under which he sank in three days.

Sectio Cadaveris.—The heart was large. The lungs gorged with blood and serous effusion. The cavity of the pleura nearly obliterated by both old and recent adhesions: the small cavity which remained was occupied by fluid. On separating the lungs from the walls of the chest, the costal pleura was found completely covered by an adventitious membrane, firm, hard, and even cartilaginous, in some places half an inch thick. The surface of this bore evidence of a more recent softer deposit, which extended upon the diaphragm and surface of the lungs. By using considerable force, this adventitious layer could be stripped off, leaving the surface of the costal pleura entire. The abdomen showed marks of extensive recent peritonitis, and contained a quantity of an opaque tenacious fluid. The parietes were, in several places, adherent to the viscera, by means of a false membrane, similar to that found in the chest, but neither so thick nor so hard. This adventitious deposit covered the liver, gall-bladder, stomach and duodenum, and arch of the colon, glueing them together, and connecting them to the abdominal walls. The small intestines and lower viscera were tolerably free from disease, but exhibited on their surface occasional patches of the false membrane described above. Two small transparent cysts were attached, by a long very slender peduncle, to the mesentery. The kidneys were very large, and, though decomposition had advanced far in them, were evidently affected with the diseased mottled structure.

Nothing can be more striking than the similarity which is observable in all these cases. I am not aware of any disease in which the character is more completely preserved, or in which the symptoms more clearly mark a specific form of malady. In the first eight cases the termination, as well as the progress of the disease, bore the most perfect resemblance; and the peculiar train of cere-

bral symptoms by which their advanced stages have been attended, have little analogy, when taken as a whole, with the symptoms of any other cerebral affection. The two last cases differ from the rest only in their mode of termination; and I have related them as the two most recent illustrations of a very frequent result of the disease.

Of the insidious nature of this malady, and its fatal tendency, these cases afford a pretty convincing proof: and the fact that so many of these have come within my own observation in a limited time, would be tolerable evidence of the extreme frequency of the disease. Yet the cases I have now detailed, but more especially the many more which the length of the present communication obliges me to defer, are chiefly such as I have, without any intention of publication, chanced to enter in my note-book, and form but a small portion of those which I have seen: but, in order to obtain a more accurate idea of the actual prevalence of the disease, it is necessary to have recourse to another species of evidence; and, accordingly, in the winter of 1828--9, I instituted a series of experiments, by taking the patients promiscuously as they lay in the wards, and trying the effects of heat upon the urine of each, and at the same time employing occasionally other re-agents. The whole number I took amounted to one hundred and thirty; out of which no less than eighteen proved to have urine decidedly coagulable by heat: and in twelve more, traces of albumen were found: giving, therefore, an avarage of at least one in six, if not one in four of the whole number. In order to show how the experiment was made, and the nature of the table I constructed, I will introduce six consecutive cases out of a male, and six out of a female ward; and it is worth remarking, that in every instance where the result allowed us to ascertain the state of the kidneys, it corresponded with the diagnosis yielded by the table. Those who had albuminous urine were found to have more or less of this disease in the kidneys; whilst those whose urine did not coagulate by heat had kidneys without disease.

Name.	*Age.*	*Disease.*	*General state of body.*	*Character of urine.*	*Effect of heat.*
John Marshall	45	fistula and phthisis	slightly emaciated	high coloured, clearly, slightly bilious	no change.
William Greatrex	51	hydrocele in inflammatory state	heated, and feverish	high brandy colour, looking quite red in large quantity	a few solid flakes first formed, which dissolved and then the urine became quite clear.
John Jones	15	lepra vulgaris	healthy	slightly clouded	clear.
Thomas Parry	36	peritonitis subsiding	reduced	natural, except a very slight cloud	no change.
Cornelius Harris	22	mercurial rheumatism	. . . . .	light-coloured	no change.
John Freeman	40	carcinoma of penis affecting the inguinal glands	pale and cachectic	fetid, with a cloud	opake first, and then forming considerable flakes.
Mary Brooks	24	anasarca, from which she has scarcely been free for three years	tolerably healthy-looking young woman, frequently having a good colour, and able to do work of ward	light-coloured dingy	coagulates into a complete curd, the thin part then looking like clear whey.
Marian Challon	29	tic douloureux and nervous affection of muscles of neck	slight in make	light amber colour	no change,
Mary Ann Williams	24	diseased ankle	. . . . .	light topaz colour	no change.
Elizabeth Welch	55	asthma	dingy, thick asthmatic complexion	deep topaz colour	no change.
Ann Simpson	13	wound in knee with glass 11 months ago	unhealthy aspect	light straw colour	no change.
Ann Macdonnal	28	suffered from two attacks of an apoplectic character	not unhealthy, no local paralysis	light straw colour, with a tinge of red	no change.

From amongst the patients mentioned in these two abstracts from the more extensive tables, three, within a short time, afforded the opportunity of ascertaining the condition of the kidneys: thus in John Marshall, whose urine did not coagulate, the lungs were full of tubercles; the liver was granular, the kidneys were healthy: in John Freeman, whose urine coagulated, the kidneys were of a pale drab colour, with decided deposit of white matter: and in Mary Brooks, whose urine was known to have coagulated for a long time, the kidneys presented the most marked specimen of the mottled disease.

In the year 1832, my friends Dr. G. H. Barlow and Mr. Tweedie undertook to make for me a still more extensive experiment, extending to nearly three hundred individuals; and the result was, that above one in eleven were decidedly affected with the disease. In the year 1835, my pupil, Mr. John Anderson, then officiating as clinical clerk, made, with the assistance of Mr. Gorham, a similar experiment on one hundred and forty-one patients; and a portion of the accurate table he drew out was detailed in a well-digested paper read before the Physical Society of Guy's. In this experiment, the proportion of cases considered albuminous amounted to above one in six; and, lastly, a more extended investigation of similar cases, made within a few weeks, and accompanied with a statement of many interesting and valuable results, from the pen of Dr. Barlow, will be found in the next number of this work.

The average of cases affected with the disease under consideration varies much, as might be expected, in different trials; and some explanations, arising from the experiments of Dr. Barlow, of which I have just spoken, will render it highly probable that the average I had taken, amounting to one in six, went beyond the reality: but with every deduction it still remains an incontrovertible fact, that the disease, in its various stages, from its earliest functional derangement to the confirmed organic malady, is one of the most frequent, as well as of most fatal occurrence: and I think I am fully borne out in the estimate which I made at the commencement of this paper, that not less than five hundred deaths annually occur in this metropolis from this single disease.

In the treatment of this formidable disease, in the early periods of its attack, I look to the circumstances with which it is most frequently connected—I mean the suppression of the perspiration, and the consequent general inflammatory condition—as most important: and by the removal of these, if the case should come under treatment before the morbid habit is established, and before the system is greatly reduced, I believe we may frequently remove the present danger: and in this state of the disease active bleeding is frequently a most important part of the treatment. I doubt whether we have it in our power, as yet, even at the earliest periods, to destroy the liability to relapse, or overcome the morbid tendency; but, at all events, the management of the early stages of the disease is easy,

when compared with the treatment in its more confirmed and protracted forms. There is no doubt that the observations of Dr. Osborne in his late valuable publication are excellent, as regards the incipient disease; but I cannot, from my own experience, entertain a hope that diaphoretics are capable of curing any large proportion of confirmed cases, even when we can bring these remedies to act forcibly and steadily. I have, on the contrary, not unfrequently found the skin exercising its function in a very tolerable degree, without any relief being afforded to that symptom which is of all the most important—the albuminous and otherwise altered condition of the urine. Till this symptom be removed, the disease certainly exists: and even when it is removed, it is often absent but for a short time, and it is for many years liable to return. It can never be sufficiently impressed upon the minds of practitioners, that the anasarca which so often occurs in conjunction with this disease is but a symptom. The disease may exist in all its force, and may be fatal, with its insidious and sudden attacks, without the effusion of a single drop of fluid into the cellular membrane, at any period of its course, and still more frequently will fatal instances be found where the anasarca, having existed, has entirely ceased;—facts which have been sufficiently demonstrated by the results of the experiments on a large scale, made at Guy's Hospital, as well as by several individual cases, both already in the hands of the profession, and brought forward in the present publication. The very interesting communications by Dr. Christison, and the observations of the zealous and deeply-lamented young physician, Dr. James Gregory, and of my friend Dr. Alison, who all very early turned their attention to this disease, have assisted to substantiate this, as well as many other points connected with its history.

The diaphoretic means to which I have generally had recourse, have been, antimonial powders, the compound ipecacuanha powder, and the liquor ammoniæ acetatis; together with close confinement to the warmth of bed, the occasional use of the warm bath and frequent fomentations, and large linseed-meal poultices to the loins and abdomen. My friend, Dr. G. H. Barlow, has thought that he has obtained almost a specific effect from the tartarised antimony: and I have certainly found it, by his recommendation, a useful adjunct to the diaphoretic mode of treatment: but in Dr. Osborne's publication, which very lately came into my hands, all the means of promoting the important function of the skin are so fully laid down and illustrated, that I cannot do otherwise than refer the reader to his excellent publication. Possibly, my want of success in producing perfect cures may depend on a less vigorous adoption of the necessary means arising from a less sanguine expectation in the result; and, as regards the hospital patients, may depend upon the difficulty of making such strict arrangements as will preclude the probability of the surface being chilled: for I have often perceived that in this disease, as in phthisis, the necessary and generally most salutary ventilation of large wards was productive of a

certain degree of evil; and I have thought that I have distinctly traced, as in the case of Maria H——, (Case 7,) the successive recurrence of the anasarca, to the effects of almost unavoidable currents of cool air passing over the bed.

With a view to keeping up the action of the skin, I have been very careful in pointing out to those who have not been confined by the disease, the necessity of wearing an inner dress of flannel at all times. I have suggested the propriety of a residence in some more agreeable climate; but I have never had an opportunity of giving a fair trial to this measure; the disease being, unfortunately, most apt to occur in those who are least able to submit to the absence from business, and the expense incident to a residence abroad. In a case which I have related above, I have little doubt that residence in a more northern climate, adopted by the patient without consultation, was instrumental, aided by imprudent exposure, in hastening the fatal result; and I feel confident that, in more than one case now under my care, the bad consequences of the disease are kept at bay by rigid adherence to the use of flannel dresses. Perhaps, to give full effect to a change of climate, some decidedly southern abode should be chosen; and a residence in one of the more healthy of the West India islands, as St. Vincent's, would probably be beneficial.

With regard to the abstraction of blood as a remedial means, it is chiefly in the commencement of the disease that it is decidedly beneficial; and at that time, combined with diaphoretics and strict confinement to bed, general bleeding, freely practised and quickly repeated, is, I believe, most valuable: I have frequently employed it at much later periods, and with temporary advantage. But when we call to mind the constant loss of albuminous matter which the system is sustaining by the kidneys, and the peculiar pallid hue which the patient usually assumes, we shall pause before we venture to afford a temporary alleviation, at a still further expense of the most nutritious and stimulating portions of the blood. Yet there is no doubt that, even in the most advanced stages, a strong tendency to inflammatory action, particularly of the peritoneum, often displays itself; and, under these circumstances, nothing but active depletion can meet the emergencies of the case. When the head is affected with pain, throbbing, or giddiness, and the senses are becoming less acute, the indication is by no means so obvious; and although we may believe that a slow inflammatory process has often been going on, yet this is, in many cases, greatly to be doubted; as the symptoms, and many of the appearances after death, admit of a ready solution, from the state of debility and anæmia to which the patient is reduced. Cupping from the nape of the neck generally relieves the symptoms for a time; but blisters are less likely to prove injurious; and perhaps a seton would be found advantageous, though of this I have no experience. When the loins are decidedly affected with uneasiness, or when pressure on the kidneys excites pain, I have known the successive daily

application of a few leeches attended by the best results, and I have occasionally introduced a seton.

There is another mode of depletion which, though it may at first appear but a temporary and mechanical expedient, I am by no means disposed to leave out of consideration: I mean, the discharge of the fluid from the cellular membrane, where anasarca exists, by punctures. It is plain, that when the meshes of the cellular membrane are distended with serum, great obstruction must be given to the capillary circulation; and by emptying them we afford the most effectual relief to the larger vessels and to the heart.

It is this interrupted circulation in the extreme vessels which is one great source of the death-like paleness which is observable on the parts distended with anasarca, and which often, in this and other forms of œdema, takes place locally, while parts of the body not affected with the serous effusion maintain a more healthy colour; thus showing that it is not simply the effect of a general want of blood. It is of great importance that all attempts to draw off the serum by mechanical means should be most cautiously conducted; for the powers of repair are weak, and there is great tendency to erythematous inflammation. The mode which has generally appeared to me most eligible is the introduction of a needle into the cellular membrane, giving it a turn or two laterally, so as to break down a few of the meshes, and thus prevent the small orifice from closing too quickly. Three or four such punctures, made in the calf of the leg or the thick part of the thigh, will give egress to a large quantity of serum, and will often continue to discharge for three or four days. I once collected in this way, in the course of four hours, above forty-four ounces of serum; and the puncture continued to flow, affording the greatest relief. The operation should not be repeated for some days; or the part should be varied; as the punctures sometimes inflame, even after they have appeared to heal. The spontaneous discharge from vesications taking place, owing to the inflammation of the surface, has sometimes afforded great relief. Dr. Barlow of Bath, who, with the wonted energy of his character, very early took an interest in the investigation of this disease, was kind enough to communicate to me several cases which occurred to him in the Bath infirmary, so long ago as the years 1830 and 1831; and, in one of these, he calculated that, for many days, a patient lost by this means some quarts daily; and the patient is believed to have recovered completely.

It is a very great question, how far the existence of albuminous urine contra-indicates the use of mercury. I have myself been led to think, with Dr. Blackall, that it is generally better to avoid the exhibition of this remedy; which opinion I find advocated likewise by Dr. Osborne of Dublin. Yet amongst the communications of Dr. Barlow of Bath, he has furnished me with one or two cases, in which he has boldly used both mercury and diuretics; and the cases have been attended with more than an average success.

Dr. Prichard, in his very excellent address delivered at a late

meeting of the Provincial Medical and Surgical Association, assures us, that for several years the question has been brought to the test of experiment in the Bristol Infirmary, whether mercury is injurious in all cases of dropsy with albuminous urine; and that it has been proved, in numerous instances where the treatment advised by Dr. Blackall has totally failed, that the same cases have terminated in recovery, under a moderate use of mercurial remedies. The authority is great, and the appeal to extensive experiment cannot but ensure most respectful attention: yet I am strongly inclined to believe, that, whatever may be the exceptions under most cautious management, the use of mercury had better, as a general rule, be avoided. When I first published upon this subject, I stated that I had seen much decided mischief from the use of this remedy; yet that I had undoubtedly seen well-marked cases, in which the free use of mercury, even to complete ptyalism, had at least not prevented the patients from deriving great, perhaps even perfect relief from the remedies with which it was combined: and, on a later occasion, I have observed that I have seen the combination of mercury, squill, and digitalis, give present relief, and produce apparent cure: yet I am bound to say that these cases are rare, while the instances of failure and obvious injury from this remedy are numerous. To illustrate this class of cases, I might refer to very frequent experience. The case of Jeremiah Collins, which I have given above, will serve as an example of what is continually occurring in the history of the lower classes; while the following case from private practice serves to show that it is not only those deprived of the comforts and conveniences necessary for the proper administration of mercury who seem to suffer from the mercurial mode of treatment, without deriving any obvious advantage.

CASE XI.

I was requested, in the month of December last, to see, in consultation with the medical attendant, Mrs. B., a middle-aged woman in easy and comfortable circumstances, who, however, had not been sufficiently careful with regard to her mode of living. She was first made aware of the decided failure of her health about a year and eight months before, when anasarca showed itself. She said she had from that time been under the care of various physicians; but had never derived any decided advantage, and was much worse than at any former period. She was now, indeed, the subject of the most general and profuse anasarca; and for several months past had experienced epileptic seizures. She was exceedingly drowsy, and had difficulty in lying down in bed. She passed about a pint of urine in twenty-four hours, which became a complete white curd on the application of heat. I found that various remedies had been employed; but, above all, she entreated me not to put her under the use of mercury, which, she said, had been tried for a long time without any good effect, but was attended with the greatest discomfort, and followed by increased debility. I

recommended her to take antimony and Dover's powder; to apply a blister to the nape of her neck; and to try an infusion of the uva ursi. Of the result of this case I know nothing; but have little doubt as to the event.

It may indeed be fairly urged, that, with such a list of fatal results before us, in which various remedies have been used, it is unjust to fix upon mercury the peculiar reproach of failure. I can only answer, that in general it has appeared to me that those who, in the confirmed forms of this renal affection, have abstained from mercury, have broken down more slowly under the disease than those who have taken it to any extent, more particularly if they have persisted till its constitutional effects have completely manifested themselves. I believe, however, that in the very early stages of the acute renal disease, mercury, given in combination with opium and antimony, and associated with bleeding, may be a useful means of reducing the inflammatory action: and I am still by no means inclined to despair of our arriving at the means of administering very minute doses of this powerful remedy, in such combinations as to lead, by long continuance, to some degree of change in the morbid structure which has taken place in all the advanced cases; or to act more decidedly in promoting the absorption of the morbid deposit, if we shall ever arrive at a knowledge of the probable period when the morbid change is in its truly incipient state. I have hoped, in iodine and in the hydriodate of potash, to find remedies adapted to the furtherance of the same object; but I cannot say that as yet my experience has borne out these hopes.

Purgatives have produced decidedly good effects in some cases, more particularly in reducing the anasarca; so that there has been reason to ascribe the chief relief, on several occasions, to their action. Amongst these, the various saline purgatives have been employed; and the combination of rhubarb with the sulphate of potash, assisted in its action by some purgative tincture. But the elaterium, under careful management, has likewise been employed, either in grain doses given once, or in the fractional part of a grain repeated after a few hours till the desired effect has been produced upon the bowels: and sometimes I have found a mild and efficient action from giving a small dose of elaterium, and following it, in three or four hours, by a dose of castor oil with three or four drops of laudanum. I have never made trial of the croton: it might possibly act well; but, like all other drastic purgatives, must be given cautiously, on account of the feeble condition to which the mucous membrane of the intestines is often reduced in this disease, and its tendency to assume a state of over-action.

With regard to diuretics, I have generally wished to abstain from all, except digitalis; and yet I have not unfrequently found myself almost forced to their adoption, when other remedies have failed, in the hope of restoring, for a time, the secretion of the kidney, which has been so greatly reduced as to threaten an entire suppression. I look upon this class of remedies, however, in the

light of a necessary evil in such cases; and do not feel authorised in recommending their employment. Very generally, diuretics seem obviously uncalled for, the secretion being already in excess.

Much as may be effected by active remedies in the acute state of the disease; in its chronic and advanced stages, particularly when any change may be supposed to have slowly taken place in the structure of the kidneys, our hopes must rest upon such remedies as produce a more gradual effect, and whose action is therefore not so obvious, and perhaps not so certain, but frequently, I have no hesitation in saying, most salutary. These remedies must vary according to a variety of circumstances, and must be persevered in for almost an indefinite length of time. Amongst these may be mentioned, small doses of carbonate of soda; uva ursi, in its different preparations; small doses of antimonial remedies; and all these, combined with some anodyne, as the conium, or, still better, the compound ipecacuanha powder. The very careful exhibition of the vinum ferri, the tinct. ferri muriatis, or some other chalybeate preparation, has sometimes appeared to do good for a time, but has not generally been admissible for a continuance. Strict attention to the state of the bowels is also indispensable; and becomes the more necessary, because their function is very apt to be interfered with, and their action to become irregular. Sometimes there is a strong tendency to diarrhœa, and occasionally quite an opposite condition exists: some mild combination, calculated to act gently and regularly, should be used; but the exact form will depend very much on our experience in the individual case, as the object should be to produce an action as nearly like the natural as possible, all unnecessary irritation being avoided.

A great deal still further depends upon diet. Where milk is grateful, if it sits easily on the stomach, and is freely digested, I believe it to be one of the best aliments which can be taken. Light animal food frequently agrees: tea should be avoided: all badly cooked vegetables, and all fruits, will often be found injurious. The great rule is, to avoid every thing which obviously deranges the stomach; and to take tonic and nutritive, but not stimulating food. The less of wine and spirituous liquors is taken, the better.

On the subject of clothing I have already said all that is necessary: let flannel be worn constantly, and every precaution be habitually adopted which may obviate the effects of whatever is calculated to chill the surface or check the perspiration. With regard to exercise, let it be gentle, but sufficient; and if perspiration be induced, which is sometimes the case, from very slight exercise, the greatest care is necessary. If horse exercise is employed, every thing like hard riding and severe exertion to the loins must be avoided. The exposure of an open carriage for any length of time, if the temperature of the air be low, and more particularly when the circulation has been previously excited by a walk, is injurious, and even dangerous.

I am aware of the difficulty of effectually enjoining all these restrictive rules; and continuing them, even when the patient

considers himself so well as to be able to live like those around him. They are however necessary if he wishes to prolong his life: this is the alternative on which he has to decide. The cases which I have given in the commencement of this communication, while they show the danger, show likewise that, with very little attention to precautionary measures, life may be prolonged in this disease for many years: but I have every reason to believe, that none of these cases approach nearly to the length of time during which the reasonable enjoyment of all the comforts of life may be continued, where the circumstances of the patient allow him to adopt, and his resolution enable him to persevere in, the means which his medical adviser may suggest.

[In a subsequent communication, Dr. Bright gives a tabular view of the morbid appearances in one hundred cases connected with albuminous urine, from which he deduces the following conclusions.]

From the analysis of the foregoing tabular view, many curious facts respecting the derangement of different organs connected with granular kidneys are brought to light; for it is most probable that a hundred cases of one disease, collected at different times and with no particular object in view, will yield results which will, in the main, be borne out by the comparison of any other equal number of cases of the same disease. The first circumstance which strikes the mind, is the extent and frequency to which the derangement of one organ is connected with the derangement of several others: yet we are not at liberty to assume that the disease of the kidney has been the primary cause on which the disease of the rest depended. It may be that some other organ has first suffered, and that the kidneys, together with the rest, have become involved. I confess I am inclined to believe that the kidney is the chief promoter of the other derangements. The only organ, except the kidney, which I think, on taking a review of the history of this disease, might probably act as the primary cause, is the skin; and this is so closely connected in its derangements with the kidney that the relations of their lesions, as regards cause and effect, become equivocal. It is however to be held in mind, that the secretion of the skin is quite as much interrupted, for a time, in many other states of disease, without the albumen making its appearance in the urine; in diabetes, for instance, in jaundice, and in certain stages of various inflammatory and febrile diseases. Moreover, it is not a fact, that in every case, or during the whole course of the disease under consideration, the skin is not perspirable, on the contrary, we often establish, for many months, the secretion of the skin, while the urine remains albuminous; as we occasionally succeed in doing in cases of diabetes, without essentially changing the character of the urine. In almost every case, the first impression which brings on the anasarca is suppression of perspiration; but it is almost as constantly the fact that the kidneys have undergone some previous irritation, and very likely that the albuminous urine, in most cases, existed previously to the

occurrence of those symptoms by which it has been recognised, more particularly previously to the anasarca.

The changes effected in the blood by the long continuance of this disease are quite sufficient to account for most extensive derangement. The extraordinary manner in which the blood becomes impoverished and robbed every successive day of a portion of its most nutritive parts must, of itself, be considered a most efficient cause of predisposition to disease; and the fact, established now by a great accumulation of evidence, and supported by the names of Prout and Bostock, of Christison, Gregory, Babington, and others, that the chemical qualities of the blood are so far changed, that urea is to be detected in that fluid, or, at all events, certain constituents scarcely distinguishable from it, is still further to be viewed as a source of disease springing immediately out of the defective action of the kidney. On the other hand, it cannot be denied that if the function of the skin is suddenly interrupted, derangements are likely to arise in various organs; and as in many instances the kidney most evidently receives a very injurious impression from the suppression of the perspiration, so other organs may be in turn affected through the same medium. I do not therefore by any means assert, that all the lesions which the foregoing table details, flow as a consequence from the kidney alone; but that they are such derangements as generally co-exist with this peculiar disease of that organ.

The principal lesions display themselves in the circulating and respiratory systems, and in the serous membranes. The heart and the lungs, the pleura, the arachnoid and the peritoneum, have, in a large majority of cases, shown marks of disease; while the liver, the spleen, the pancreas, and even the intestines, have frequently been, to all appearance, in a state of health, and have comparatively seldom given proof, by their structure, of any peculiarly diseased action. Of all the membranes, the pleura has decidedly been most often diseased; but that disease has, in forty cases, consisted of old adhesion; which, though it might have been connected with the first attack of renal disease, or might have taken place at some later period, in connection with that affection, may probably only mark the liability of the individual to be affected by atmospheric changes, and may have been the result of some casual inflammatory attack. At all events, the twenty-six cases in which the pleura was apparently healthy, and in three of which its freedom from disease is distinctly stated, prove that however general a limited inflammatory action of the pleura may have been, it forms no essential part of the disease. That the pleura is however liable to inflammatory action, in a large proportion of these cases, may be inferred from the sixteen instances of recent inflammation; while the serous effusion, which has occurred in forty-one cases, has been connected with that general loss of balance between the actions of the exhalents and the absorbents which is obvious in every part of the system.

The same tendency to disease which is manifest in the pleura,

shows itself, though in a less degree, in other serous membranes. In the pericardium, we have found six instances of old adhesion, eight of recent inflammation, and twenty-three of serous accumulation; and in the peritoneum, ten instances of old adhesion, twelve or thirteen of well-marked, recent, and often most acute inflammatory action; and twenty-three of the effusion of clear serum, in three of which a false membrane had been formed by chronic action: and again, looking to the arachnoid, we find that membrane rendered opaque, probably by a more or less severe inflammatory action, in thirteen cases; while well-marked serous accumulation had taken place beneath it in twenty-nine cases, and had partially distended the ventricles in six.

The deviations from health in the heart are well worthy of observation: they have been so frequent as to show a most important and intimate connection with the disease of which we are treating; while at the same time there have been twenty-seven cases in which no disease could be detected; and six others, which, from not having been noted, lead to the belief that no important deviation from the normal state existed. The obvious structural changes in the heart have consisted chiefly of hypertrophy with or without valvular disease: and what is most striking, out of fifty-two cases of hypertrophy, no valvular disease whatsoever could be detected in thirty-four: but in eleven of these thirty-four, more or less disease existed in the coats of the aorta; still, however, leaving twenty-two without any probable organic cause for the marked hypertrophy generally affecting the left ventricle. This naturally leads us to look for some less local cause for the unusual efforts to which the heart has been impelled: and the two most ready solutions appear to be, either that the altered quality of the blood affords irregular and unwonted stimulus to the organ immediately; or, that it so affects the minute and capillary circulation as to render greater action necessary to force the blood through the distant sub-divisions of the vascular system. The valves chiefly affected have been the semilunar valves of the aorta and the mitral; and in three cases the tricuspid has been somewhat deranged. In three cases, likewise, the disease of the valves has been unattended by any hypertrophy of the heart.

It is observable, that the hypertrophy of the heart seems, in some degree, to have kept pace with the advance of disease in the kidneys; for, in by far the majority of cases where the muscular power of the heart was increased, the hardness and contraction of the kidney bespoke the probability of a long continuance of the disease. Six cases are noted, in which the heart was soft and flaccid, and four in which it was unusually small; and in most of these, though not in all, the disease of the kidney had not proceeded to the state of contraction and hardness.

The principal diseases of the lungs have been œdema and bronchitis, frequently attended by an emphysematous condition of certain portions. Œdema has occurred in thirty-one cases; and it is very commonly the immediate cause of dissolution, or of the increased

distress towards the approaching termination of the chronic form of the disease. In six cases, recent, and five old, traces of pneumonia were found; while the embarrassment to the circulation, caused by these various diseases of the heart and lungs, had occasionally given rise to the effusion of blood into the tissue of the lungs, in the form which is now known by the term of pulmonic apoplexy. The instances in which phthisis, or any form of scrofulous or tuberculous disease has been connected with the renal affection, have been decidedly rare; so that in only four cases has recent phthisis developed itself: and what is somewhat remarkable, in more than double that number the disease seems to have made a certain inroad upon the upper lobes of the lungs, and then to have sunk into a state of quiescence, or entirely subsided: from which we should perhaps be inclined to infer, that so far from these diseases being associated, the condition of the body in this form of renal disease is unfavourable to the existence of phthisis, or that it is certainly not peculiarly apt to occur in tuberculous constitutions.

With regard to the liver and the abdominal viscera generally, as compared with the heart and lungs, a very great immunity from structural disease is to be observed; a fact the more remarkable, as the habits of intemperance with which the renal disease is so frequently connected are those which might be expected to act very directly on the liver and digestive organs: indeed, to this day the impression is so strong, as to the injurious effects of stimulants being manifested chiefly on the liver, that the majority of practitioners no sooner see the bloated countenance of anasarca connected with the history of intemperance, than they proceed to consider in what way the depraved action of the liver is to be corrected, and its morbid changes retarded. Looking to the tables before us, a very different conclusion forces itself upon our mind, as to the condition of the liver in general anasarca, and in that state of cachexia which often attends upon intemperate habits. We here find, in thirty-one cases, the liver distinctly stated to be healthy; and in nine other cases, so free from all suspicion of deranged action, as to be pointed out as remarkable specimens of the healthy organ; thus making forty in the hundred free from disease. In thirty-two cases, any deviation from the natural appearance was exceedingly slight; and was, in a large proportion of them, nothing more than that mottled state which is derived from the irregular distribution of blood throughout the texture—a condition very frequently observed, where the circulation through the chest is obstructed. The instances of confirmed diseased structure did not amount to above eighteen. There seemed to be no marked connection between the condition of the kidney and of the liver; for nearly one half of those cases which were stated to be remarkably healthy were coupled with the hard and probably most-advanced form of the disease, while the other half occurred in cases apparently less advanced; and the more severe cases of hepatic derangement accompanied every variety of the disease in the kidney. The

only two instances of fatty degeneration in the liver were in cases where the kidney was soft, smooth, and white; but in another, where the liver was somewhat fatty, the kidney was hard, rough, and lobulated.

The stomach seems, in many cases, to have suffered from the excessive use of stimulants. In eighteen cases, the effects of irritation on the mucous membrane has been recorded: and as this is an organ which is more likely to pass unnoticed than the liver and some others, it is probable that this number would have been increased if its condition had been more constantly or accurately examined and noted.

The spleen and the pancreas have very generally been mentioned as healthy.

The intestines have, in several cases, though not very generally, shown marked signs of disease. In about nineteen, the small intestines have been irritated in some portions of their courses—in a few of these, ulceration has taken place; and in seven cases the colon or cœcum has been diseased; but several of these have occurred in conjunction with tubercles in the lungs, and have therefore been scarcely ascribable to the peculiar circumstances of this disease.

The diseases in the substance of the brain itself have chiefly consisted of that unequal distribution of blood which is apt to produce a mottled appearance when the medullary substance is exposed in slices, and which is frequently attendant on convulsive or apoplectic seizures. In some cases the brain has been exsanguine; and in a few the results of such lesions as the rupture of vessels may induce, have been observed.

The foregoing table likewise affords an instructive average of the immediate causes of death in this disease. I have been able to trace the circumstances connected with the conclusion of life in seventy cases; and find that no less than thirty out of these seventy have died of well-marked symptoms of cerebral derangements, noted under the titles of "apoplexy," "coma," "convulsion," and "epilepsy." Eight others have died suddenly. In eight cases, the obstructed condition of the lungs has been the immediate cause of death; and in three, the effusion into the chest has hastened the dissolution. Next to head affections, the most prevalent diseases have been inflammatory attacks in the serous membranes; amongst which are five well-marked cases of peritonitis, three of pericarditis, one or two of pleuritis. Diarrhœa and other exhausting diseases have carried off several; and in every case, except two or three, the death appears to have been the result, not of casual disease, but of such events as may be said strictly to belong to the condition of the kidney of which we have been treating.

One other point suggests itself, as capable of some illustration from the foregoing table—the period of life in which most have fallen a sacrifice to this disease, and the probable degree to which it shortens life. In seventy-four cases, the age has been recorded;

and of these, four only have survived beyond their sixtieth year; thirteen have passed their fiftieth year; but few of them have lived to fifty-five: twenty-one have died between forty and fifty; sixteen have passed thirty years; and nineteen have died before they had arrived at their thirtieth year: and if we take those who have died in their forty-fifth year and below that age, we find that the large proportion of fifty out of seventy-four have sunk before the meridian of life. The youngest whose age is given is only eight; and there is one advanced to seventy-three: showing, therefore, that neither youth nor age is exempt from this disease, but that it has cut off the greater part of its victims before the middle period has been attained.

LECTURE

ON THE

GRIPPE, OR EPIDEMIC INFLUENZA.

BY M. ANDRAL.[1]

TRANSLATED FROM THE FRENCH BY THOS. F. BETTON, M. D.

This disease has frequently appeared as an epidemic, with various degrees of intensity. Recently, the name of Influenza has been given to it. One of its principal characteristics is to attack simultaneously a great number of individuals; so much so, as to spare but few in those countries which are subjected to its ravages.

It likewise extends over a large tract of territory, and is not circumscribed, like other epidemic maladies. It has not always presented the same symptoms: at times it has assumed a very severe form, and at others a milder character. The whole of the symptoms characterising it have never been observed to occur in isolated individuals; or, in other words, this affection has never been known to assume the sporadic form.

Epidemic influenza may appear at intervals more or less remote. Thus, in France, it has been seen three times in six years—in 1831, 1833, and 1837. The first two visitations were less severe, and attacked fewer individuals, than the last. The special cause of the influenza is some agent which we cannot detect. It has been observed in damp countries, subject to great variations of temperature; as it extended, it invaded warm and dry localities; it has appeared and raged with equal severity in all seasons: hence, it would seem, it may occur without regard to any peculiar atmospherical phenomena--there is some unknown cause influencing its production.

In Paris it spared neither age nor sex. However, but few

[1] La Lancette Française, Fevrier 28ème, 1837.

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children under the age of two years were affected by it; it attacked a large number of patients of the age of twelve years; its maximum of frequency was amongst adults, and the old did not escape—cases of the disease having been observed in those who had attained the advanced period of eighty-five years.

It appeared in all ranks of society, and in all the various localities; and raged with equal violence in the most populous and filthy districts, as in those inhabited by the most opulent classes. In some very rare instances, one member alone of a family was attacked, the remainder escaping the disease; in others, all the individuals were affected, either simultaneously or consecutively. Is the influenza contagious? This is an exceedingly embarrassing question. Reasoning from analogy, its propagation by contagion might be granted. Persons labouring under organic lesions of the lungs, phthisis, and pulmonary emphysema, &c., are more liable to contract the malady than others.

SYMPTOMS.

A remarkable symptom in this disease is a certain disturbance in the nervous system, affecting particularly that of sensation and motion, and but rarely involving the mental faculties. There is cephalalgia, more or less intense, from the onset; the patient appears overcome by it—he can scarcely move his head. The pain is chiefly seated about the middle of the forehead, and is compared by the sufferer to a lancinating pain, or a sensation of extreme heat or weight: it is accompanied by dizziness and vertigo. Some persons suffer exceedingly, and declare that, if it continues, they must die of *cerebral fever* or *apoplexy*. At the same time the face is flushed, the eyes injected and filled with tears; the ears are troubled by an unpleasant buzzing sound. Some have experienced a pricking sensation in different parts of the body, principally in the palms of the hands, which in some cases appeared perfectly natural, and in others were red, swollen, and erythematous, especially at the articulation of the last phalanges with the metacarpal bones.

In others, the pain was less superficial, and was manifested by deep-seated lancinating sensations in the limbs; in others, again, by dull feelings, especially about the great articulations. The pain was generally felt from the commencement of the attack; the cephalalgia and weariness, in a greater or less degree, have been, in some cases, the only symptoms experienced by the patient; in other cases, there existed a sensation of constriction of the thorax, preventing the dilatation of its parietes, and menacing suffocation. In no case, however, was this phenomenon alarming.

Thus far we have treated of the disturbance of the apparatus of sensation—let us now consider that of motion. We have universally observed depression of the muscular power: this symptom continued sometimes throughout the whole course of the disease, and existed even after the disappearance of the other morbid phe-

nomena. In some fortunately rare cases, there were very painful cramps, which gave the patient excessive uneasiness. With very few exceptions the intellectual faculties presented nothing worthy of remark. The patients were much depressed, but the mind remained clear. In some individuals labouring under intense fever, a transient momentary delirium was observed.

APPARATUS OF DIGESTION.

In the great majority of cases, the tongue was broad, moist, and covered with a whitish coat; nevertheless, in some it was red and dry. The fauces presented no remarkable appearance. The pain of the throat of which some patients complained was not seated in the pharynx, but in the air-tubes. Deglutition was always performed with ease. Want of appetite was felt during the period of fever, and sometimes continued after the subsidence of the other symptoms. In general the thirst was not very great. Some patients complained of a sensation of weight at the epigastrium, rarely of intense pain. In a large proportion of cases the belly remained soft and indolent. The alvine evacuations were natural, and constipation was more frequent than diarrhœa.

In some cases, however, whether pertaining to the disease or to its complications, there were observed very obstinate vomiting and copious diarrhœa, accompanied by colicky pains. These phenomena were rare at Paris, but this appears to have been the most common form of the influenza at Passy.

APPARATUS OF RESPIRATION.

The disturbance of the functions of this system was numerous and varied. In a great number of cases, the mucous membrane of the nose was the seat of mucous and sanguineous discharges. At times the hemorrhage was slight, at others so copious as to require plugging the nasal fossæ. The voice was almost invariably altered; more or less hoarseness, and pain in the course of the larynx and trachea, were the characteristic phenomena of the epidemic. A cough was rarely absent, and frequently ushered in the attack, the remaining symptoms being, as it were, grouped around it; in several individuals it was very obstinate, continuing for a long time after the disappearance of the other symptoms. It sometimes returned in very painful and fatiguing paroxysms. It was sometimes dry, sometimes followed by expectoration of ropy, whitish, viscid, transparent matter, containing globules of air. In some cases the sputa were more consistent and opaque, resembling those at the close of acute bronchitis. Sometimes, also, the cough continued for a longer period, and was accompanid by acute suffering, threatening instant suffocation.

The seat of this cough was in the larynx and trachea, rarely

low down in the bronchi; hence, in a large majority of cases, auscultation and percussion gave merely negative results. In some cases, however, a sonorous dry rattle was heard, indicating obstruction and thickening of the bronchial mucous membrane. At times the respiration was normal; at times was observed dyspnœa of greater or less severity. In some, this embarrassment of respiration proceeded from very acute pain of the thoracic parietes, impeding their dilatation. Accompanying the dyspnœa, there were sometimes seen greater or less change in the features, a violet hue of the face, and coldness of the extremities. These symptoms, however, when energetically treated, promptly vanished; death never resulted from them. The oppression has been more or less considerable, when secondary inflammation of the lungs and pleura manifested itself. We must also remark, among the causes of the dyspnœa, the pseudo-membranous inflammation of the bronchi, pointed out by M. Nonat. This alteration is not extremely rare, and would be more frequently observed, if, in autopsies, greater attention were paid to the examination of the minute ramifications of the bronchi. In old persons, an extremely copious secretion from the bronchi was observed, which, as they were unable to expel it by coughing, excited all the symptoms of catarrhus suffocativus.

CIRCULATORY APPARATUS.

Fever was absent in a small number of cases, in which all the other symptoms of influenza were manifested. In an immense majority of cases, the skin was burning, sometimes dry, but more frequently moist, or bathed with perspiration; the pulse frequent, full, bounding; the pulsations of the heart strong and full.

The duration of the fever was from two to five days, rarely exceeding the latter period: if it appeared after the fifth day, there was danger of inflammation of the bronchi, or some other portion of the organs of respiration. In some persons was observed a singular tendency to lypothymia and syncope. It will be understood, from the exposition of these different symptoms, that the influenza may be perfectly mild, or present a certain degree of severity.

SECRETIONS.

With the exception of the cutaneous exhalation, mentioned when describing the disturbance of the respiratory apparatus, the secretions evinced no remarkable change. In some cases, the perspiration was very copious, appearing with the fever, continuing with it, and after its disappearance resembling the *sweating sickness*. Sometimes the skin was merely moist: in some cases, a copious perspiration, on the second or third day, coincided with the cessa-

tion of the febrile action. A miliary eruption sometimes appeared with the perspiration.

PROGRESS.

Three stages may be observed in the course of this affection. The first, which may be wanting, is marked by cough, pain in the throat and head, and dull pains in the limbs. If the pain in the head and cough existed alone, these symptoms would not constitute the influenza; in order to be assured of the presence of this disease, some of the nervous symptoms first mentioned must have been present. According to circumstances, we have the nervous symptoms predominating in the thorax, or in the abdomen. This stage lasts from one to two days; when it is wanting, the disease commences with the febrile stage. This is marked by fever, accompanied by most of the symptoms, which, we have said, are its attendants. This stage sometimes follows imperceptibly the first; but more generally occurs suddenly, whether it has been preceded by the first, or whether it has of itself opened the scene. Thus, the cephalalgia increases, as also the debility and prostration; some individuals are, as it were, struck by lightning, either while walking, or while attending to their business. This stage lasts nearly as long as the fever which precedes rubeola or scarlatina. The third stage, named apyretic, is marked by cessation of the fever, with which, however, all the symptoms rarely disappear. Certain symptoms pertaining to other stages still continue. These refer to the functions of the central, thoracic, and abdominal organs. In the encephalon, we have cephalalgia and prostration; in the thorax, cough. As a general rule, this last remains, in a greater or less degree, for many days: for example, it still exists (latter part of February) in patients who were attacked at the commencement of the epidemic (latter part of January). Although, in the first two stages, the stomach may have evinced no signs of disturbance, this frequently occurs in the third stage. The tongue is covered with a thick coat; the mouth is clammy and bitter; the thirst is not intense; the patient, on taking food, complains of a disagreeable taste; and, after its ingestion, the stomach swells, and becomes the seat of an inconvenient sensation of weight. Constipation is more frequent with these symptoms than diarrhœa.

The duration of this stage is very variable; it sometimes terminates in two or three days, and sometimes lasts for a longer period. The symptoms disappear gradually, and convalescence commences.

In the last epidemic which still visits us, the influenza has terminated, in a very large proportion of cases, favourably. When death did occur, it was from the complication of other diseases, as pneumonia, general bronchitis, &c. Nevertheless, let us suppose that the symptoms we have detailed be carried to a greater degree of intensity, and we will easily understand the fatal results observed in other localities.

The prognosis of influenza, varying according to the prevailing character of the epidemic, was favourable in Paris, with the exception of individuals labouring under chronic inflammations of the chest.

The influenza, like all epidemics, was not always accompanied by all its symptoms: some individuals were but slightly affected by it, or presented but an isolated symptom, or a single series of symptoms; they are then said to labour under an incomplete or imperfect influenza. Examinations after death revealed merely the lesions peculiar to the diseases which existed, as complications of influenza, but none which were proper to that disease.

After having recapitulated the symptoms of influenza, let us ask what is its nature. Is it laryngitis, tracheitis, or pulmonary catarrh? We think not. Inflammation of the larynx, trachea, and bronchi, may indeed appear in the influenza, and be one of the principal elements of the disease, but cannot constitute it entirely. Again, the autopsical characteristics may be wanting. The influenza is, therefore, a general affection, the nature and cause of which are unknown, as are those of the greater part of epidemics which appear at irregular epochs.

TREATMENT.

The treatment must vary with the symptoms. When we observe signs of cerebral congestion, and a greater or less degree of fever, we should not hesitate to open a vein, and at the same time apply revulsives to the extremities. If the fever be mild, the headache not severe, the oppression slight—rest and the use of demulcent drinks will suffice. If the mouth be clammy or bitter, the tongue coated, with inappetency for food and a sensation of weight at the epigastrium, an emetic (18 or 24 grs. of ipecacuanha, or 2 grs. of tart. ant. et potass.) may be administered with advantage. Should the cough be dry and harassing, the narcotics—such as belladonna or opium—ought to be preferred.

In the second stage, if no particular symptoms appear, we should insist on diet and mucilaginous drinks.

In the third, if the symptoms of gastric or intestinal disturbance predominate, an emetic, and especially purgatives, are exceedingly useful. During convalescence, we have found it necessary, in order to restore the languishing functions of the stomach, to administer bitters and tonics.

MEMOIR

ON THE

PROPER METHOD OF EXAMINING A PATIENT,

AND OF

ARRIVING AT FACTS OF A GENERAL NATURE.

BY M. LOUIS,[1]
PHYSICIAN OF LA PITIE, MEMBER OF THE ROYAL ACADEMY OF MEDICINE.

TRANSLATED FROM THE FRENCH BY HENRY I. BOWDITCH, M. D.
Fellow of the Massachusetts Medical Society, Member of the Society for Medical Observation at Paris, &c. &c.

The physicians of antiquity have left us but very imperfect descriptions of the diseases which they observed: they have handed down to us numerous rules in therapeutics, but have given no proofs of the truth of them; their doctrines have been supplanted by others, all of which have pretended to be the only true ones. Modern physicians have been but little more successful; their doctrines have passed away the more rapidly, according as the spirit of investigation has made progress; and their descriptions, at least for the most part, are so incomplete, that even the individual cases which they have detailed in their works present so much of uncertainty about them, that we cannot always convince ourselves that the disease really deserves the name which they have applied to it. For this reason, their observations, with a few exceptions, cannot be of any aid in the advancement of science, nor can they serve for the instruction of any who may read them.

Nevertheless, among the physicians of antiquity, as well as among their successors, until the present time, there have been many men of great celebrity and of rare powers of mind, in whom, in fact, there seemed to be nothing wanting to enable them to advance science, especially since pathological anatomy has been allowed to be studied without restraint. How happens it, then, that science does, in truth, owe so little to them, and that its history

[1] Mémoires de la Société Médicale d'Observation de Paris, tom. i. art. i. Paris, 1837.

is, in many respects, only the record of their errors or of their systems?

The same questions might formerly have been asked in regard to chemistry. Doubtless, this department of learning had many learned men among its votaries previously to the last forty years; nevertheless, it is only within this last period that chemistry has made rapid progress. What means has it employed of late which were not used before? It has demanded exactness; it has weighed and counted always when it was able to do so; it has taken rigid notice of *every thing* which had any bearing upon a question; it has substituted a strict analysis for an imperfect and careless one; its methods have become daily more and more precise, and its progress is rapid and constant.

The same cause which kept chemistry so long in its infancy—viz: the want of rigid method—has weighed upon the destiny of medicine, and prevented its growth. Of this assertion we may easily be convinced.

In fact, it has been acknowledged, from time immemorial, that medicine is a science of observation; nay, it has been said that it consisted solely in observation--that is to say, it has been allowed that nothing can be done in medicine save by means of well-observed facts. This proposition having been once admitted, it would seem as if there were but one thing to be done—viz: to observe with great care, and *without distinction of cases*, all the facts of the science; to consider each patient as a problem to be solved, and for the correct solution of which we must collect the greatest number of data that we possibly can; consequently, to make enquiries relative to *all* the functions during life, to describe *all* the organs after death, and, after having collected a larger or smaller number of such facts, to analyse them with care, and deduce what consequences we can from them. This process would have been slow, it is true, but sure, and very similar, if I mistake not, to that pursued by the chemists, who in their analysis take *every* circumstance into consideration. Instead of this, and as if the principle had been admitted without any conviction of its truth, only very incomplete facts have been collected. During life, all the functions have not been scrupulously investigated, and after death the condition of all the organs has not been carefully examined; in other words, we have tried to solve a problem without collecting all the data in reference to it.

Moreover, physicians, taking for their observation those facts alone which seemed to them the most remarkable, and limiting themselves to the rigorous analysis of these observations, could not know more than one of the phases of a disease, and that commonly the worst one; in other words, that which is most distinct from the common course. But, as they have not confined themselves to the strict analysis of their recorded facts, a portion of their descriptions rests solely on their memory, and the most exact of these are evidently imaginary, either wholly or in part. Thus we find that, although physicians, from the earliest period, have declared medi-

cine to be a science of observation entirely, they have in fact paid very little attention to observation.

If observation had been regarded in the light in which they pretend it was, they would soon have remarked the difficulties which it presents—the time that it requires; they would have said so, and would have devoted themselves to it exclusively, during a certain number of years at least. And who among them has spoken of the difficulties of observation? One would say, during the perusal of their works, that there was nothing easier than to precisely determine a fact: and the venerable Pinel himself seems to think that every thing depends upon a good classification; that, by aid of a good nomenclature, by means of which he could hold communication with his pupils from one end of Europe to the other, the study, and doubtless the perfection, of medicine would no longer present any difficulty. Pinel likewise, as many less celebrated physicians have done, did not hesitate to confide to young pupils the task of collecting facts, in order that he afterwards might depend upon these facts as upon a solid basis. Entrust the care of collecting facts—that is to say, of laying the foundation of a science—to young men almost without any experience, many of whom, I doubt not, were persons of good talents, but not one of whom could feel the immense importance of the labour committed to his charge! We should laugh at a chemist who should engage one, just entering upon the career of chemistry, to make his analysis for him;—what must we think of these physicians who make use of cases of disease recorded by young pupils?

It was a circumstance so wholly out of the common course, for any one to collect cases after having left the medical school, that when I began, fifteen years ago, to make a regular series of observations, I was looked upon with surprise and pity, so that it required some degree of fortitude to be able to meet this twofold sentiment.

I refer to this fact, in order to be better able to characterise the period at which it occurred, and in order that the cause which has contributed most to retard the progress of medicine may be no longer doubtful in the mind of the reader.

Thus, on the one hand, we find imperfect observation, and on the other the habit of making analyses which are incomplete or dependent upon facts entrusted to the memory; and these are the causes to which we must attribute the imperfection of medical science. Let us, therefore, first determine what we must do in order to render observation as complete as possible, at least in the actual state of things.

CHAPTER I.

Conditions to be observed in making observations.

The proper examination or observation of a patient consists in endeavouring to ascertain the condition—not of one of his organs, for then we should learn only about a part of a whole—but we must study to obtain information respecting all his viscera, or (speaking more generally) *all the parts* which compose the individual. But as we cannot generally know the condition of an organ save by that of its function, it is evident that we must investigate all the functions, if we would know the actual condition of a patient.

It is important, before proceeding to the study of his functions, to make oneself acquainted with the usual mode of life of the patient, his age, customary food, profession, quantity of flesh and strength, the diseases to which he is liable, and the good or bad formation of his frame. Let us examine the reasons for the above statements.

We ought to know, first, the age of the patient, because the same diseases are not equally frequent or dangerous, do not follow th same laws at all periods of life, and because there are some which are not observed after a certain period: secondly, the profession or trade of the patient, because there are diseases which belong almost exclusively to certain professions; for instance, the lead colic to that which requires the employment of this metal; trembling of the limbs to that of gilding with metals, &c. &c.; and in order to be able to deduce all that is important in relation to this point, we must know how long the patient has pursued a certain profession, and whether he has done so with or without interruption, &c. &c. Thirdly, his customary food, under which term I include the quantity and quality of the aliments—a twofold circumstance which may have great influence upon the course and development of diseases, even those which are sporadic, but which cannot be incontestably demonstrated except by means of numerous facts. Fourthly, the degree of strength or debility, whatever may be the cause of the latter, because the degree of strength has a great influence upon the course, termination, prognosis, and treatment of diseases, especially those of an inflammatory nature, whose ravages are more rapid, more severe, and more inevitable, as it were, just in proportion as the patients themselves are more debilitated. Fifthly, his regular or irregular course of life, excesses of every kind, since they necessarily have a debilitating effect upon the constitution. Sixthly, the usual state in regard to embonpoint, or the reverse, for the double purpose of ascertaining, by means of time and similar facts, the influence of those conditions upon the course and development of morbid affections. Seventhly, the disease which he may have experienced previously to the present attack, in order, on the one hand, that we may be able to appreciate rigorously the influence exerted

by the strength or debility of the constitution upon the frequency of morbid affections, and, on the other, that we may know those among them which are most liable to return, or which are observed most frequently at this or that period of life. The reader will perceive how much precaution and quickness of intellect are requisite in him who makes these questions, in order that certain results may be arrived at. Let a patient, for example, declare that he has had a pneumonia at some remote period antecedent to the actual disease; the examiner should not rest satisfied with this simple declaration. In order for one to be certain of the fact, the patient must recollect and be able to indicate the characteristic symptoms of the disease, and the examiner ought not to admit any thing more to be true than what a memory of ordinary powers can recall. Hence the power of this last faculty should be noted in each particular case. Eighthly, as to the necessity of making oneself acquainted with the good or bad form of the chest, it depends upon the fact that the same diseases do not seem to have the same degree of severity in one who is well formed and another who is not so, &c. &c. This last particular can only be decided by means of a great number of facts.

We must not, however, forget, in these examinations, that we are not seeking merely to verify the assertions made by different authors; that the object of an observer is to gain knowledge; that he ought, therefore, to study each patient under the greatest possible number of points of view, whether these points have or have not been mentioned by previous writers.

When we have once determined the above facts, we must settle precisely the *time* at which the disease *commenced*, for without this we cannot know either its duration, or its course, or have all the elements necessary for a correct prognosis and treatment, or even those of other questions which it may be important to solve.

The exact period at which acute diseases commence, it is usually easy to determine. We must not, however, remain satisfied with the first answer the patient may give upon this point, for generally it is inaccurate, as most patients pass by, as of no importance, the light symptoms which so often precede more serious ones, and fix the origin of their disease at the moment when they experience some severe suffering, or are obliged to give up work. In order to be sure of arriving at truth upon this point, we should, after having asked of the patient how long he has been ill, learn from him whether he has experienced any pain or uneasiness in any part of the body; whether he had previously more thirst or less appetite than usual; whether he had cough, &c. &c.; in a word, we must ask questions in relation to *all* the functions. When we pursue this method, we often find that the health of the patient has been a deranged one, or, perhaps, many days previously to that on which he says his disease began. Or perhaps we find that, for many days, he has been suffering from an affection entirely distinct from his actual disease; whence arises, necessarily, a modification in the ideas we may have formed in relation to the present complaint,

its gravity, or the causes of many of the phenomena we may have observed in it.

Suppose, for example, that in a case of erysipelas of the face, the patient should fix the time of the commencement of the disease at three days before the examination is made. Let us suppose that, previously to this period, when swelling, redness, and pain, began in the skin, &c. &c., the individual has less appetite, more heat of the external surface, and more thirst than usual, together with lassitude of the limbs, &c. &c., while, at the same time, no deeply seated organ appears to be affected in any sensible manner, we should be obliged to refer the commencement of the disease to the period at which these last symptoms first made their appearance, and to draw the conclusion that an affection wholly local, in appearance, may be preceded by general symptoms which can neither be explained nor referred *entirely* to this local disease, even when they arise at the same moment. Thus we find the local disease is not the same in all affections. Let us suppose another case, pleurisy, for example, the first symptoms of which can be referred back only to a few days before the patient comes under our notice. Now, if this patient says he was very well previously to this attack, and if his declaration be considered as true, the prognosis must be favourable, since observation proves that a pleurisy which occurs in healthy subjects has a favourable termination. If, on the contrary, as we see it happen every day, it results from more precise questions being made to the patient, that already at the commencement of the pleurisy a cough had existed a certain time; if a more serious examination shows that this cough is connected with the existence of crude tubercles, what a difference we should make in our prognosis! There can be no doubt, that it is owing to this superficial examination of patients that physicians have derived the opinion that pleurisy is a very grave disease. It is likewise from the fact of not distinguishing the cases of erysipelas of the face, that arise in persons who are perfectly healthy, not debilitated by age, or other causes, from those which occur under opposite circumstances, that physicians have supposed this affection also to be very dangerous. For out of one hundred and twenty subjects, whom I have observed, or whose history I have studied, and whose health was perfectly good at the time the erysipelas began, no one died, even in those cases in which the disease ran successively over the whole, or nearly the whole, surface of the body. I may state as much relative to gastritis in similar circumstances; and I should infer from all this, that one of the most important points necessary to be attended to in the history of acute diseases, is the condition of the patient at the moment of their appearance. Are there many observations which, in this respect, deserve much confidence?

In determining the time of the origin of chronic diseases, we meet many difficulties; but, with a little patience, we can surmount them, and we arrive at truth, or very near the truth, by aiding the memory of the patients; that is to say, by recalling to them the chief events which have occurred during the course of the year, or

of previous years—the seasons, the great changes in the atmosphere, any public celebrations which have occurred within a certain time, &c. &c. But in doing this, we must be very careful to receive as well authenticated facts only those which do not suppose an extraordinary memory, which facts are much more common than we are at first led to think. Thus, for example, a patient might very well remember the beginning of a disease of eight or ten years' standing, provided that commencement was marked by slight or grave symptoms, if, since that time, the person has been obliged frequently to keep in bed, and has always worked less than before. For how is it possible for one of the labouring classes to forget when the disease began which has brought so much, so long, and such constant suffering, and such an important change upon the condition not of one person only, but often of a whole family? All that is necessary in order that a circumstance may recur vividly to the memory after a long space of time, and that it may be received as true, is this, it must have forcibly impressed the mind at first. Nevertheless, we must take every precaution, in order to be assured that there is no error. For example, almost all women remember the first menstrual period.

The time at which the disease begins having been fixed, we must pass to the *examination of the symptoms*, study them singly in the order of their development, from their first appearance up to the moment of the patient's being submitted to our examination; determine precisely their degrees of severity, their regular or intermittent course, especially the exact time of their commencement, for without this attention we shall fall into the most serious mistakes. Suppose, for example, that in a hundred cases of the typhoid affection, observed with but little accuracy, pains in the abdomen and diarrhœa should have been present from the beginning of the disease in forty only, we necessarily should be led to conclude that precursory symptoms are much less frequent in this affection than in other acute diseases of our climate. But suppose, on the contrary, that from one hundred similar cases, collected with care, pains in the abdomen and diarrhœa have appeared with the first symptoms eighty times, we should infer, not less rigorously than before, that, with respect to precursory symptoms, there exists no very great difference between the typhoid affection and other acute diseases. What different results are those to which we are led! I would remark, likewise, that if the origin of each symptom were not fixed with a very great degree of exactness, we should be unable to form a correct idea of many phenomena; among others, of those which have been called critical, and those observations in which they should be noted as such would be of no use whatever in this respect.

Moreover, as it is only by the study of functions that it is possible to discover the affected organ or organs, it is evidently necessary, as I have already stated, to examine all the functions, otherwise we shall be liable to have an incomplete and frequently incorrect history of the disease we may wish to know.

However, in the actual state of science, it would not be sufficient to determine accurately the time at which the symptoms began, their course and degree of severity. In order to complete the study of them, a new degree of exactness is necessary, the importance of which will be easily seen, and which I will briefly mention. Suppose, for example, the abdomen is, or has been, the seat of pain; it is then necessary not merely to fix with precision the character and time of commencement of this pain, but we ought, if it be possible, to indicate the spot at which it commenced, since that place may be the part where the disease itself began. This we see in the typhoid affection, in which the pain in the abdomen begins generally in the right iliac fossa, that is to say, in the part which corresponds to those patches of Peyer which are first diseased. As much might be said of pains in the chest, of auscultation and percussion of the same, and for similar reasons. It would be entirely insufficient to notice, for example, that the thorax gives a dull sound on the right side, either anteriorly or posteriorly; we should indicate the precise spot where this dull sound occurs, whether at the top, or at the base, or at the sides; and we should likewise note with care the exact extent and degree of it. The same reflections apply to the different *râles*, to the subcrepitant in particular. If one were to limit himself to the mere fact of its existence in acute pulmonary catarrh, for example, in which we observe it so frequently, it would be almost doing nothing, or, at least, passing over in silence one of the most important points in the history of this *râle*. Observation, in fact, teaches us that, at the beginning of the disease, this *râle* is heard only at the posterior and inferior portion of the chest, whence arises a very marked difference between this disease and phthisis. By studying daily the subcrepitant *râle*, and noting exactly the part of the thorax where it ceases to be heard, we find soon that it extends from the base to the summit of the lung, uninterruptedly; and thus we are able to prove, by the only means which observation now possesses, that pulmonary catarrh is propagated from one part to another, like other affections of the same nature.

The history of symptoms, according to the sense and manner which have just been mentioned, is indispensable; and if one proceeds in an inaccurate or too rapid manner, neither diagnosis, prognosis, nor therapeutics, can, to a certain degree, be possible. The last, in particular, cannot make any progress, since the application of any method of treatment whatsoever presupposes the exact determination of the cases in which it has been employed with success, or, in other words, great precision of diagnosis. In fact, every thing depends upon this labour, whether we be at the bedside of the sick, or in the silence of the closet, when we want to deduce from particular facts the laws which govern them. We cannot, therefore, be too careful upon this subject.

Doubtless, those individuals who perceive but very slightly, as yet, the wants in medicine, and who do not clearly see the causes which have retarded its progress, will consider the method I have

prescribed as the only good one, to be very long and very minute, especially when compared with those observations which have been made *in general terms* by the ancients, and have been transmitted to us, and always considered of so much importance, and which still satisfy the imagination of those who are unacquainted with our art, by the rapidity of their style, and the strength of their expressions. But these observations, which have been so much boasted of, and which are so concise, are, in fact, so very concise, that generally it is quite impossible to determine whether the disease which is mentioned be really the one with which the patient was afflicted, and in none of them can we be certain whether it was simple, or complicated with something else; so that, as I have already mentioned, these cases can be of no utility whatever.

Those physicians, however, who are most satisfied with them, and who have a kind of aversion for detailed facts in medicine, never think of blaming the natural philosopher who, when making his experiments, notes accurately the state of the barometer, thermometer, and hygrometer, &c. &c.; all of which circumstances appear very insignificant, but the philosopher takes notice of them in order that he may draw just conclusions from a rigorous examination of the observed facts. And how can a physician draw correct conclusions, if he does not take into account all the data which can, or ought to, enter into the solution of the problem which he proposes to solve?

But this sentiment, which leads individuals to admire the observations of some of the old writers, and to regard, with a kind of disdain, all detail of facts, has led to another unfortunate result. I refer to the fact, that some have recorded only those facts which are remarkable for some peculiar symptom, or cases whose course has been somewhat different from usual. In this way, as I have already stated, a part of every description of disease necessarily depends solely upon facts entrusted to the memory, or, in other words, is entirely imaginary. I could cite, in defence of this assertion, which is not exaggerated, the fact that physicians and surgeons of the Paris hospitals, of a deserved reputation, thought, for a long while, that they cured the majority of their patients affected with a certain disease, or who had been operated upon, and they were very much surprised when, by data which they could not contradict, it was proved that the contrary was the fact. However, it is easier, any one will allow, to remember the severity of the disease, its mortality, than its different symptoms, their degrees of intensity, their times of commencement, duration, &c. &c., all of which circumstances we must have constantly present before the mind, if we would describe any disease with exactness. How, then, can we have confidence in any description which is not the rigorous analysis of well observed and accurately recorded facts?

When observation is thus regarded, that is, when it is *looked upon as a serious matter*, and as the sole means we have for the solution of questions which may present themselves to our mind,

observation then is *difficult*, and presupposes a true apprenticeship to be pursued, such as every profession requires, which calls for any degree of proficiency for its proper performance. Only a very few persons are able to practise it; those who can pursue it without bias, and who have, as it were, a detestation of whatever is merely an approach towards truth, and who feel that the science, whose object is the preservation and re-establishment of health, should not be cultivated with less care and exactness than natural philosophy and chemistry. If, in truth, one will but reflect upon the difficulty of learning the truth of facts, and of recording them with exactness, and upon the number of observations which are necessary in order to the thorough investigation of any subject of science, he will be convinced that the art of observation can scarcely require less than all a man's powers.

I have said that observation was difficult, and required a thorough apprenticeship. In fact, how much habit and attention are needed in the examination of patients, that nothing essential should be omitted; how much care in proposing our questions, so that we may not lead the patient to return an answer different from what is true; to be able to distinguish those which are the result of lassitude or of ennui from others, made with attention, and consequently to be regarded as the expression of real facts; how much too is required, in order for one to be able to practise with success the various methods of examination of the chest! Another mode of investigation, not less important than the others, and which fixes their just value, is pathological anatomy. How much habit and perseverance does not this suppose, in order that it may be a useful instrument in the hands of him who employs it!

Pathological anatomy determines the just value of the other modes of investigation, and it explains them. Without pathological anatomy how could we know that a fine crepitation is the mark of the first stage of inflammation of the lung? that gurgling and pectoriloquy are owing to tuberculous cavities? ægophony to an effusion of fluid into the pleuræ? and that symptoms of softening of the brain are owing really to this affection, &c. &c.? It is evident that we could not know any of these points without the aid of pathological anatomy. It is very true that we should be able to recognise the seat of many diseases without its aid, and by means of symptoms alone, but how could we know their nature save by means of pathological anatomy? The symptoms of apoplexy indicate well that the brain is the seat of this affection. It is the same with those of softening of that organ. But how could we know that the former depend upon a hemorrhage—while the latter are owing to a softening, to an inflammation of the brain—without the aid of pathological anatomy?

Moreover, as the history of symptoms is of no use, except when they have been collected with great accuracy from the beginning until the end of the disease, and all the functions have been examined in a like manner, all the *positive* and *negative* facts noted with equal care, so likewise pathological anatomy cannot render

those services to science which one would expect from it, unless we proceed with an almost extreme care in the examination of every organ in all the subjects who die, and note the exact state in which it may be, whether natural or unhealthy.

Pathological anatomy, in fact, has for its object not merely to determine the seat of diseases, their nature, their verification and complications, but it likewise is the sole means for arriving at the knowledge of a great number of perhaps the most important laws of the animal economy; and this knowledge can only be the result of an examination equally attentive of *all* the organs in patients who die, whatever may be the cause of their deaths. If, for example, we know that after fifteen years of age we do not find either tubercles or gray semi-transparent granulations in an organ, without, at the same time, finding them, and commonly in a more advanced state in the lungs, it is to pathological anatomy that we owe the information, and to pathological anatomy studied in the mode I have mentioned. For if one were to content himself with examining at each autopsy the lesions of that organ which is principally or exclusively affected, it would be impossible, even with a thousand autopsies, to arrive at the fact I have stated, the importance of which none can doubt. It is in the same manner, and by means of similar researches, that we have learned that ulceration of the pharynx, œsophagus and small intestine, are peculiar to two affections, (I except syphilis,) one—acute, severe continued fever; the other—chronic phthisis; likewise, that ulcerations of the larynx and trachea, especially the latter, with the above mentioned exceptions, occur among phthisical patients only; and this is almost the case likewise with a fatty state of the liver, &c. The laws settled by pathological anatomy become still more important than we should be led to think them from this fact, when we find that these laws, in certain cases, and independently of the symptoms in their absence, and, in truth, sometimes in opposition to them, can lead us to a rigorous diagnosis. Peritonitis, when of a chronic character from its commencement among adults, that is, between the ages of fifteen and a very late period of life, is, according to facts which I have recorded, constantly tuberculous, or connected with the existence of gray semi-transparent granulations, developed either upon or under the peritoneum. But, as I have already stated, neither of these lesions exists in any organ unless it is observed likewise in the lungs, so that when there exists a case of well marked chronic peritonitis, we are able, independently of the symptoms referable to the respiratory organs, or even in their absence, to recognise the existence of phthisis, or, in other words, the development of more or less tubercles, or gray semi-transparent granulations in the lungs. We ought to do so, for if the law we have just stated admits of some exceptions, they are very rare, and the law will not be the less certainly established. I have more than once announced the existence of phthisis in patients who presented all the symptoms of chronic peritonitis, but neither auscultation nor percussion of the chest afforded any signs of an appreciable alteration of the pulmonary

parenchyma, and this even in patients who had not any cough. This diagnosis many may consider as having a very slight foundation, and others may think it very bold, but it was a just one, however, and I could not give it up without, as it were, denying the laws of the animal economy, and science itself; for these laws are science.

Nevertheless, whatever may be the care we may take in the examination of organs, there will be some cases, from time to time, in which we shall find, when opening the body, no lesion capable of explaining death, or the symptoms which have been observed during life; and facts of this nature, of which there can be no doubt, have probably led to the conclusion that pathological anatomy had not all the importance which we generally attribute to it. But these cases are, in fact, precisely those which prove in the most evident manner its indispensable necessity, since if all the viscera have not been examined with scrupulous care, we could not be certain that there was no lesion capable of explaining at once the symptoms and the fatal termination of the diseases in these cases.

Whether, therefore, pathological anatomy demonstrate or do not demonstrate the reasons of the phenomena observed during life, or the cause of the fatal results of diseases, its importance remains the same. To decide otherwise would be, as it seems to me, to deny evidence, and to maintain that because different affections are sometimes latent, the study of symptoms is therefore of no importance. I do not fear to say that pathological anatomy has been neither too much boasted of, as some declare, nor too much depreciated, as others say, but its uses have often been little understood. It is a mode of explanation which no other can supply; it is no other thing, and it certainly is of much worth, and because it is one mode of learning about diseases, a mode of verification applicable to all diseases, it seems to me we ought not to make it a science by itself, any more than diagnosis and prognosis.

But it is not sufficient to have stated the condition merely of the patient, to have studied all his functions from the commencement to the termination of the disease; we must likewise collect all the data which can or may lead us, at some time, to the knowledge of its occasional or remote *causes*. This is a very important subject, new as it is and surrounded with difficulties, which however we must enter upon even with little hope of arriving at any result; for how many observations are necessary to determine, amidst the circumstances in which man lives, those which must have had some influence upon the development of his morbid affections! It is important likewise, because from the profound knowledge of it we must derive the means of escape from such or such diseases. It is new, as I have said, because there has been scarcely a single author who has not introduced a description of a disease by a long list of causes, which are almost always the same. But admitting that similar causes may produce diseases the most various, still we ought to have a proof of the fact; but we have by no means this proof, nor even the elements of it. It is evident that these cannot

be sought for, save in those things which exert necessarily an action upon man; and in this research, as likewise in all others, we must not only pay attention to what authors have stated, but we must enquire into many circumstances which they have not thought of. When the facts are collected, we may be able to distinguish the influential circumstances from those which appear to have no bearing upon the production of the disease.

Predisposing causes, that is, those which gradually prepare us for the reception of diseases, or which modify them, are perhaps very numerous; I say perhaps, for what do we know that is positive in this respect? In order to know these facts, we must note in each particular case the age, sex, degree of strength or debility, temperament of the individual, his susceptibility, his degree of intelligence, manner of life, and the changes it has undergone during several years, whether his habitation be dry or moist, the state of his clothing, the more or less frequent use of baths, and the passions of the individual. We should likewise learn the age of his father and mother at his birth, and their diseases, since many morbid affections appear to be hereditary, and this hereditary nature of diseases ought to be more studied than it has been of late; for, in order to demonstrate it, we must not only ask of the patients we examine the most common diseases of their parents, or those of which they died, but should first learn whether their diseases were acute or chronic, and then determine about the existence of the principal symptoms which must have developed themselves. For example, suppose we wish to learn whether the patients died of an organic disease—cancer of the stomach, for instance—we should ask whether there was long-continued vomiting, considerable emaciation, and what was the colour of the matter vomited. If the question arises as to the existence of aneurism of the heart, and if one of the patients' relations appears to have been affected with it, we must not remain satisfied with this declaration, but we should ask whether a more or less marked œdema was connected with the oppression in breathing, whether there have been palpitations, and for how long a time they have existed, &c. &c.; and if the patient is unable to answer precisely these questions, we must consider the fact as of no importance at all with reference to the hereditary nature of the disease; for we should never forget that as consequences, in order that they may be admitted as such, should be rigorously deduced, so to be true they must depend upon exact facts as their basis, and no doubt must arise in relation to them. How, for example, can we put entire confidence in the assertion of a patient, who should say that his father died of phthisis, if he is unable to give any account of the length of the disease, and of the principal symptoms which have accompanied it?

However, as our object should be not to prove, diminish our confidence in, or modify what has been stated in regard to the hereditary nature of diseases, but to know the truth upon the matter, we must not content ourselves with mentioning the disease of which the patient's parents died, when it is similar to that of the

individual whom we are examining, but we must describe their diseases, whatever they may be.

The study of the occasional causes, or of that of the facts necessary to arrive at them, seems much less difficult, these facts being more or less recent, and for that reason more easily recalled. However, this enquiry has its difficulties, among the most prominent of which we should place the prejudices of patients. Thus, sciatic pains are often excited by moisture, or rather by a moist, cold atmosphere. Almost all patients who are afflicted with this complaint, refer their pains to this cause; but if we insist upon details upon the point, we find that this cause is very often imaginary—that the patients had attributed their disease to it, from the belief that it could not have originated otherwise. As much might be said in regard to taking cold in cases of phthisis, and for similar reasons.

But it may be urged that cases loaded with such details cannot be read, or at least are almost illegible, and that this fact shows the folly of the method which adopts it. To this objection, which is not an imaginary one, I would answer, that the question is not whether the details I have mentioned would be pleasant to be read, but solely, or at least before every other circumstance, whether they be necessary in the research after truth. The affirmative of this latter question cannot be doubted; therefore we ought to collect our facts in the mode just mentioned. I would add that, with time and patience, details, however numerous, may be made in an observation without its becoming of an intolerable length; and by arranging them according to some fixed plan, the reader who wishes it may read some parts and pass over others, so that the observation may be as short and as long as one may wish.

Let us suppose, for example, that we would have the history of an acute disease; one should place in the same part, the age, profession of the patient, &c. &c., the origin of his disease, and the symptoms which he may have experienced from this moment until that at which he comes under the inspection of the examiner. In a second part, we might detail his present condition, and in a third, all that relates to causes. Relative to ulterior facts, we should make as many divisions as there are principal epochs in the disease, and in each one of them we should relate, successively, the condition of each function, by indicating the dates of any important changes. It is evident that when once this kind of classification is admitted, each observation may be as long or as short as one may wish, and, as I have already stated, be more or less read, according to pleasure.

A similar method might be followed with chronic diseases.

In fact, this species of reduction, or of classification (*redaction*), requires much labour on the part of him who atttends to it; but it is the only one, in my opinion at least, which allows of the union of brevity with detail; and the uniformity which it imposes, and which might, at first, be regretted, is the sole means of preventing a great loss of time for the reader.

The details into which I have entered, will doubtless appear

very numerous, although I have omitted all those which a slight experience shows us to be necessary. But we must not forget that we are speaking of the foundation of our science—of the part which requires the most trouble—of a preliminary which cannot be imperfect without science becoming vain and imaginary; in a word, we are speaking of observation, and therefore we must allow that these details could not have been abridged.

CHAPTER II.

Method to be pursued in order to arrive at general conclusions from individual facts.

In this undertaking, as well as in the preceding one, we must always remember that we must not guess at the truth, but find it; that nature is a problem which cannot be solved except by means of a sufficient number of exact facts; that to assert a fact which is not the rigorous analysis of those data would probably be lending to nature a language which she disclaims, and very certainly advancing a statement which cannot be proved, and which, consequently, cannot be considered as true. Therefore, the complete enumeration or rigorous analysis of all the exact facts of the same kind which we have been able to procure, is the sole means of arriving at a knowledge of the laws of our economy. This precept logic requires of us, and custom rejects as of too troublesome a nature.

The first condition which it is necessary should be attended to, is this—to collect those facts only which are exact; that is to say, those only which have been recorded according to the principles previously laid down, and with the care and in the detailed manner which we have already stated, remembering that if, in the record of a case, the writer has limited himself to the diagnostic symptoms or lesions that are peculiar to the disease which he thinks he has been observing, we should be led to doubt whether the brevity of these cases be not dependent upon the rapid manner in which they have been observed; for when we are rapid in making an observation, we can hardly hope to perform this duty with accuracy, even if it be but small, and however much talent we may possess which may fit us for the labour.

When we have collected a sufficient number of exact facts, we must group them, bring into one class those which resemble one another and indicate the existence of the same affection, and separate those which present opposite characters. In order to do this, we must not only consider the symptoms in themselves, but likewise examine their course, duration, mode of succession, and the different circumstances amidst which they are developed. Thus enteritis, properly so called, usually begins with numerous liquid stools, as in the majority of cases of continued fever; so, at the

first glance, without making more than a rapid examination, one might be tempted to place the facts relating to these two affections in the same group. But, except in this one particular, these two diseases present so many differences that it is impossible to confound them. The classification of facts becomes more sure and very much easier, when among those of the same kind there are some relating to subjects who have died. Then, in fact, there can be no doubt as to their similitude or identity, if the same lesion occurs constantly in all, and if it be not observed in any other affection.

After the facts have been classified, we must study them; and as for each disease we have stated the condition of all the functions and organs, we must now study all the symptoms and organs in all the particular facts. We must do this, because in the same disease we do not observe simply the lesions or symptoms peculiar to the organ primitively affected, but because there are many others which are met with more or less frequently in affections the most diverse, and without which we should have but a very imperfect idea of the disease.

It is of little importance which symptom we begin with; for I suppose we commence with the study of them. The first thing we wish to learn is, whether this symptom occurred in all the cases we are to analyse, and if not, in what proportion it has occurred. In other words, at the very outset of our study, and when analysing the symptoms, we are compelled, as it were, in spite of ourselves, to count. We are compelled to do so, unless we declare that, in order to know a disease, it is unnecessary to become acquainted with all its elements, to know whether a symptom is of frequent or rare occurrence, whether it occurs in all or only a great many of the cases. Nevertheless, there are physicians who deny the necessity of this method, who even declare that there are difficulties in its accomplishment, although they daily employ the expressions *rare* and *frequent*—expressions which would seem to show that the individuals had counted; but which, in reality, prove one thing merely, viz., the folly of those who employ such expressions, since although they deny the necessity of counting, yet make use of expressions from which one would infer that they in fact had used this method.

Doubtless, the fact of counting merely is not sufficient to enable us to know the relation facts bear to each other; and by the employment of the numerical method, without discernment, we might be led into absurd results, or those which experience would never verify. For example, in order to know the greater or less frequency of occurrence of a symptom, it is not sufficient to count the number of times it has occurred in a certain number of individuals, but we must determine whether this proportion is the same for all ages and both sexes, and in the various circumstances in which the different individuals, whose history we may be studying, have been situated: in order to know these differences, also, we must count.

Between him who counts, in order to analyse rigorously, and him who has not counted, and who uses the expressions more or less, rare or frequent, there is all the difference that there is between day and night, truth and error; a clearly demonstrated and perfectly scientific theory, and one which is vague and of no value; for how can we give any value, in scientific investigations, to any thing which is vague?

No one denies the necessity of a considerable number of facts to the knowledge of a disease, and to give an exact description; but of what use are they, if we do not count them?

Other persons go farther, and oppose the numerical method, because the number of facts upon which it rests is always limited; and, say they, in order that it may be of the value which its friends give it, it would be necessary to have a much greater number of observations than it is possible for one man to collect. But this objection, which contains more than one error, as we shall very shortly perceive, is one of the strongest arguments in favour of the numerical method; since, as each observer will count, numbers that are limited, added to others similar in respect to amount, will finally become very considerable; so that the law, or even the exact amount of the law, which will be the expression of these accumulated facts, must of necessity be rigorously exact.

We are unceasingly speaking of the experience of ages in regard to medicine; but how can this experience be a reality, if those who write—instead of saying I have seen so much, or so many times—have used the vague expressions, I have often, or have rarely, seen? If the reverse of this had been done, had they all used precise expressions, the experience of one might have been joined with that of another; but how can we connect the experience of the writer who has used the terms, more, less, frequently, rarely, with that of one who has used the same terms? Imagine, for instance, a thousand authors who have pursued this latter course; it is just the same as if but one of them had existed, or rather, in some respects, as if we had never heard of any of them. If, then, there is any method for learning what is the experience of ages, it must be the numerical method alone.

Under all circumstances, every one will allow that in order to decide upon any subject we must understand it, and to draw any advantage from what our author writes, we must understand him; now I would ask, what the words more, less, rare, and frequent, mean? Frequent—does one mean by this word twenty, thirty, fifty, or sixty times in a hundred? Who can answer this question?

Let not any one, however, suppose that a very considerable number of facts is necessary to establish a law. M. Benôiston de Chateauneuf endeavoured to learn the influence of prosperity in life, or of that position in society which allows us to provide fully for our wants, upon longevity. M. B. could not procure more than six hundred well authenticated facts relative to persons who had enjoyed, during every period of their lives, the greatest ease in

regard to pecuniary circumstances. The analysis of these facts led him to the establishment of a law, and this law was still farther confirmed afterwards by the examination of a much larger number of facts, and with only a slight difference in the proportions. I, likewise, when analysing two years since the facts which I had gathered at my clinical visits during the four preceding years, found the same laws to exist in them that I had deduced previously from a much larger number of cases. In this manner I have really gained experience, whereas by any other method I should have been unable to have learned what there was in common among facts collected at considerable intervals of time; I could not have added result to result, verified or criticised one by the other.

After studying the proportion in which certain symptoms occur, we must attend to their duration, their degree, their origin, &c. &c. If the same symptom should occur in all patients affected with the same disease, at the same period, we should know the time of its commencement by what would occur in a particular case; but as such is not the fact, we necessarily must take the mean time if we would know the time at which a symptom commonly begins, and afterwards state the periods which are most remote from this mean time, and likewise the frequency of its occurrence before and after this, unless indeed we are willing to say that the expressions, more, or less, near the origin of the disease, which are daily made use of, are much more definite than numbers.

It is evident, likewise, that as the degree and duration of a symptom are not the same in all patients, we must, if we would know them in a general manner, employ the numerical method; that is to say, in order to study in a satisfactory manner, to fully decide any point in the history of a symptom, we must make use of addition, without which there can be nothing except what is vague and unintelligible.

What is necessary to be done for the study of symptoms, we must likewise do as rigorously for that of the organs of those who die, whether our object be to explain facts—for, after all, *counting is only one mode of doing this*—or to avoid numerous grave mistakes. Suppose, for example, we are treating of the autopsies relative to individuals who have died of the fever or typhoid affection, which has been of late considered as being an inflammation of the stomach and intestine, in a word, a gastro-enteritis. If such, in fact, is the nature of the disease, we ought to find the stomach much more frequently diseased in those who die of it, than in those who suffer from other diseases, and always inflamed when the affection is rapidly fatal. Let us count, in order to decide the question. This has been done, and the stomach has been found to be perfectly healthy in individuals who have died rapidly, for instance, after seven or eight days of disease; likewise, that it is not more frequently inflamed in typhus than in consequence of the most diverse acute diseases. Hence it has been concluded, rigorously and necessarily, that typhoid fevers are not cases of gastro-enteritis; that is to say, all that was necessary was to count accu-

rately the facts which were collected, and thus discover an error which could then be very easily avoided.

This is not all; for as a symptom is not equally frequent, severe or slight, at all ages or in the two sexes, the same may be the case with many lesions; but how shall we know any thing in reference to this point unless we count? And no one certainly will think this knowledge to be of little importance, since, if this difference exists, it would suppose a corresponding one in the course, and, if we may so express ourselves, in the genius of the disease, according to age and sex. Thus by counting, it has been decided that the fatty change of the liver takes place much more frequently in phthisis than in any other chronic disease—in woman than in man; but the inverse of this law holds good for ulcerations of the epiglottis, larynx, and trachea. Now suppose that a man of genius, I mean one that dreads counting, has before him two thousand autopsies of every kind, among which facts relating to phthisis must necessarily be numerous in both sexes, how could he arrive at the knowledge of the facts which I have just mentioned?

But it will be said, these facts of a general nature, these anatomical laws of which you speak, have been deduced from a very limited number of individual cases, and how can you be certain that they would be the same if you were to study upon a larger number of facts? To this question I will answer by asking another: How can we decide whether these laws be true or false, unless we begin by counting, and adding one result to another?

If an organ is the seat of many lesions, it becomes important to decide which of them is the most frequent, and we must therefore count. It is owing to the neglect of this, and to the fact that we have yielded to the custom of depending upon memory, that we must attribute a multitude of false assertions and apparently ingenious theories upon imaginary facts. Thus Corvisart, after having declared that aneurism, with diminished thickness of the parietes of the right ventricle, is much more frequent than active aneurism of this cavity, undertakes to prove by a train of reasoning that it could not have been otherwise, and that it must be very different with regard to the left cavity. Corvisart does not state how he proves the truth of the assertion which he tries to explain. The reader may seek for the proof in his work, but where will the proof be found? Out of fifty cases of aneurism of the heart, reported by that celebrated physician, there is one case of aneurism of the right cavities with diminution in the thickness of their parietes, and even these were not measured. It may be answered to this, that Corvisart had not observed those cases merely, the histories of which he has published, but that his assertions rest upon a much larger number of facts. But even in this case Corvisart should have counted, and if he had done so, he would have stated the fact, since the question was simply one of number. I would remark, that the simple statement of this result would not have greatly augmented the size of the work; it would even have tended to shorten it, since by it he would not have been obliged to explain what was, in fact,

merely imaginary. The proportion of cases of dilatation with diminution in the thickness of the parietes of the right cavities of the heart, is not greater in the work published by MM. Bertin and Bouillaud upon diseases of the heart. Out of fifty cases of organic affections of the heart, collected by myself at La Charité, there is not a single example of dilatation with diminution of the thickness of the parietes of the right cavities; and hypertrophy with dilatation was even more frequent in the right than the left cavities. I would add to what I have already said, that I have, for five years past at La Pitié, seen but one case of thinning of the right cavities of the heart; so that we see that the opinion of Corvisart, adduced as a fact, is not correct.

But in order that the importance of symptoms or lesions may be known, it is not sufficient to know their severity, their times of commencement, and their frequency. We must likewise strive to decide whether like symptoms or lesions do not occur during, or in consequence of, other diseases; we must study them in all the details we have mentioned, and again count, and afterwards compare the results. This labour is extremely arduous and of immense importance, and the results can be obtained in no other way. It is a labour which supposes that in each particular case the condition of all the functions has been noted, and that the examiner has not limited himself to the history of those which were more or less seriously altered; in a word, that the positive and negative symptoms have been recorded with equal care.

It will be said, likewise, that the symptoms of diseases are not exactly the same at all seasons of the year, and in every country; that they are modified by epidemic influences; and, therefore, it is useless to count. It may in fact happen, that the proportion and gravity of the symptoms are not always the same in sporadic and epidemic diseases; that analogous variations occur in sporadic diseases which happen at long intervals, &c. &c.; but this last fact cannot be rigorously demonstrated except by counting, so that this objection is, in truth, one of the most decisive arguments in its favour.

After the study of symptoms, comes that of the *course* of the disease. This is very important, for the diagnosis of a disease depends entirely upon this, in certain cases. It comprehends the knowledge of the duration of the disease, that of each symptom in particular, its violence or mildness, its duration, time of commencement, whether intermittent or constant; and this course of the disease may be varied by the age, sex, degree of strength of the patient, and by his being addicted to excess of any kind. We must strive to solve all these problems.

If, as it has been already stated, when speaking of symptoms, the duration of diseases was always the same in all cases, whether severe or trivial, the age and sex being the same, a single observation would be equivalent, in this respect, to thousands; but as the case is not really so, we cannot learn the general length of a disease except by taking the mean character from a great number of indi-

vidual cases, taking care always to mention the extremes of the duration, the number of cases composing these two classes, and the mean time itself. All this presupposes the act of counting. We thus see, that, as we advance in our study of the means by which we are to arrive at general facts, it is impossible to succeed unless we employ the numerical method; and that without it we can have nothing more than a mass of isolated facts—that is, we can have no laws and no science.

It is but seldom that authors have counted until now. I would ask, what disease is there with whose duration we are accurately acquainted, if we except some eruptive diseases, in which we do know it imperfectly? Books on pathology, it is true, say that age, temperament, &c. have an influence upon the cause and duration of diseases, but where is a rigorous truth of the proof of this assertion? Who can tell the measure of this influence? We can find it no where; and how is it possible to arrive at it by any other means than those just mentioned?

And how can we know any thing relative to the favourable or unhappy termination of diseases, and their severity, unless we count? For nothing really has been said in the assertion that it is cured more or less rapidly or slowly, or that it brings death more or less frequently, since one may easily imagine that this unfortunate termination occurs in the third, or fourth, or a tenth part of the cases; yet how different are these results! And if mortality varies according to the age, sex, strength of the patient, and the seasons of the year; if it is not the same in villages and cities, in places remote and near rivers, in warm and cold countries, or in the same place at different epochs, &c., can we know this, except by counting the number of times that death occurs in a certain number of persons in the circumstances which we have mentioned?

The relative frequency of diseases is a subject of not less importance than the preceding, and it requires as exact observations, and supposes necessarily that during a certain time all the cases which have presented themselves in a general hospital have been recorded. It is well known that pneumonia is more frequent than nephritis, but exactly in what proportion this is the case we are entirely ignorant. Inflammations of serous membranes are not rare, but we are unacquainted with the different proportions in which each occurs. This knowledge, however, is by no means superfluous, since it would show what differences there really are in tissues which seem similar in every respect; it would probably overcome many prejudices, and would show us the degree of dependence or of independence of diseases of the viscera, of whatever nature they may be, and of the membranes covering them.

We should remark likewise, that if these affections called organic are influenced by medical constitutions; if they are not equally frequent during periods of the same extent; if the two sexes are not equally affected with them; if the same difference is observed at different ages of the same sex—we must have recourse to the

numerical method, if we would learn upon what to depend on these points.

The study of *causes* is one of the most difficult, as well as one of the newest, which can present itself to the consideration of a physician. How must we proceed? First, we must make two classes of influences to which the patient has been exposed; one near, the other remote from the time of commencement of the disease. Afterwards, we must study them separately, endeavour to discern their duration, frequency, and severity. Consequently, we must count the cases in which these influences have been exerted, after we have grouped them, and examined them under a certain number of aspects. If one of them has occurred only ten times in a hundred, for example, we might consider it as nothing; but we should have a very different opinion, if it had existed eighty times in the same number of cases; especially if it have not occurred in any other disease so frequently; for in the appreciation of causes as in that of symptoms, we should always compare the affection which we are studying with others.

Doubtless it might happen, that after having carefully studied the different circumstances in which the patient is placed, we might not arrive at any useful result; but at least we should have avoided any errors which, in any other mode of procedure, would have been inevitable. The history of phthisis proves this. How can we believe, in fact, that phthisis is the result of long or frequent inflammation of the bronchia, or of the pulmonary parenchyma, when we know that pulmonary catarrh, at least when it is of any severity, more frequently occurs in men than women, in the proportion of three to two; that such is likewise the case with inflammation of the substance of the lung, while the exact reverse of this is true in phthisis; likewise, that tubercles are not more frequent in the lungs of those persons in whom the bronchia have been red, swollen, dilated for one or many years, than in those in whom the bronchia have presented none of these lesions.

It appears, therefore, that it is indispensably necessary to count, if we would be able to appreciate the true value of symptoms, or of lesions; know the usual course and duration of diseases, determine their degree of severity, their relative frequency, and the influence of medical constitutions of the atmosphere upon their development; or know any thing relative to their causes. Numeration is a condition, without attention to which it will be impossible to arrive at any precise result, or to explain facts. Without it, we can neither make any laws, nor establish any science.

We now arrive at the last, the most important, and, doubtless, the most difficult, part of our labour, viz., the just appreciation of the effects of therapeutic agents, and the determination of their action, whatever that may be; for it is not sufficient, for this purpose, to study the immediate effect of the therapeutic agent. We must notice chiefly its effect in relation to the mortality, and the more or less rapid course of diseases towards a favourable or unfavourable termination. In order to arrive at this object, we must

compare together a great number of cases of the same disease, of equal severity, some relating to subjects in whom the disease was left to itself, others of individuals to whom certain medicines were given. After doing this, we must study the action of the same therapeutic agent on those in whom the disease was severe, and on those in whom it was slight—on those in whom the remedy has been used in large or small doses, at a period near to, or remote from, the commencement of the disease. This last circumstance is very important. So, likewise, we must mention whether the medicine is used alone, or in conjunction with other remedies. But not only does this method require much labour, it also supposes a considerable series of facts, the connection of which is difficult, especially when treating of severe affections in which we are used to making new attempts frequently, and which will not allow of our remaining a spectator merely of the progress of the affection. For it must be evident, that we do not seek to know, by approximation, what remedies have *appeared* to be more or less successful, but to demonstrate, in a rigorous manner, that a certain remedy, or certain method, is useful or hurtful, and in different degrees, according to the manner in which we employ it.

The facts necessary for the solution of the problem which we propose, must therefore be provided with numerous and exact details. We should not be contented with merely stating (as has been done in some recently published works) that in a certain ill-recorded case leeches were employed, no mention being made of their number; that this application was made at the beginning, or very shortly after the beginning of the disease; and that afterwards a large venesection was performed, &c. &c. Such an observation ought not to be employed in such a question as we are now studying. For the term, beginning of a disease, is a vague expression, and really does not signify the first more than the second day; neither is the expression, "a great number of leeches," any more definite; and as much might be said of the words, "afterwards" and "large venesection," for they do not indicate with precision either the quantity of blood that is drawn, or the period at which it is taken.

When a large number of exact facts has been collected, if we would learn the therapeutic effect of any remedy, or collection of remedies, taking into consideration the age, sex, temperament, strength, or debility, we must learn whether, under these various circumstances, a greater number of patients were cured by one remedy than by another. In other words, when we have collected our facts, we must count them; and our present uncertainty in regard to therapeutics is, in a great measure, owing to the circumstance that no one has, until now, counted. It is owing to this, likewise, that we do not really know just how far a remedy is useful or otherwise, even when we are satisfied that it is somewhat beneficial.

These views, I am well aware, have seemed very strange to more than one person; and some individuals have allowed that they might be all very well in theory, but they were very different when

put in practice by the bedside of the patient. Let us, therefore, examine, and see whether the objections that have been urged against the numerical system, when applied to therapeutics, are as valid as they are thought to be, and whether it is possible for us to arrive at any exact conclusion in reference to therapeutics, by any other method, or without its aid.

The first, and, apparently, the most serious objection to this method is, that it is difficult to collect a sufficient number of cases of any disease; for instance, of pneumonia, which we may call identical, especially if we remember that perhaps there do not exist two cases exactly similar of any affection.

Doubtless, if it is true that, in order for two diseases to have this similarity, they must refer to persons of exactly the same age, same degrees of strength, same stature, *embonpoint*, &c. &c.; if the two must have the same length (supposing every one could measure it exactly); if the febrile excitement must be the same, so that the arterial pulsations may not be more numerous in one case than in another, even by two or three strokes merely; if such are the conditions, it is very certain that we never could collect any cases that would be similar, any more than on the same tree we could find two leaves exactly alike in form, colour, and thickness. But as it must be allowed that, in order to draw general conclusions, we must collect similar facts, it would follow that in medicine there could be only individuals; that it would be impossible to come to any general conclusions; and, likewise, that we should be unable to describe, in a general manner, the leaf of a tree. Happily, experience teaches us the value of these deductions, and consequently that of the assertion whence they are derived. A leaf of a tree, when once well described, may be always recognised; and the general facts in pathology, when once firmly established, may be verified daily in circumstances similar to those in which the subjects were, from whom these facts were deduced. It follows, from this, that we can really collect facts sufficiently similar to enable us to deduce laws from them which experience will daily confirm.

Reasoning *a priori*, as the opponents of the numerical method have done, we might infer, from the diversity of temperament, size, intelligence, and many other external circumstances easily appreciated in man, that an equal amount of differences would be found in the internal organs, and in their modes of action. We might think, for example, that the stomach would need as many different kinds of food as there are individuals. Nevertheless, experience demonstrates that, notwithstanding the minute differences which every one must see in individuals the most alike, ninety-nine in a hundred of those who differ in age, temperament, sex, &c., make use of the same food, prepared in the same way.

Experience demonstrates, likewise—and to experience we must appeal from all reasoning—experience demonstrates that the same remedy administered in the same disease to individuals presenting the greatest variety of ages, degrees of strength, temperament, &c., may have nearly constant success. For instance, drastic purges

in colica pictonum, quinine in intermittent fevers, &c. Whence it follows that, on the one hand, cases, in order that they may be grouped together, do not all require an absolute or imaginary resemblance; and, on the other, that when the action of a therapeutic agent is very efficacious, it exerts itself, notwithstanding the numerous differences which exist among those submitted to its operation, for these differences seem momentarily effaced by the disease itself.

It may be answered, with reference to the quinine, that the reasoning is not decisive, as, according to the supposition, this remedy has been exhibited in intermittent diseases. But of what value is this objection, when we consider that the individuals to whom it was administered, have presented numerous differences in regard to age, sex, temperament, degree of strength, &c. &c.?

If it is not indispensable to have regard to many circumstances in order to appreciate, in a general manner, the effect of agents in resisting lead colic and intermittent fever, we may likewise abstain from them in other diseases. Suppose, for example, that in any epidemic disease five hundred patients, taken indiscriminately, have been submitted to a certain mode of treatment; that five hundred others, taken in the same manner, have undergone a very different treatment; ought we not to conclude, if a greater number died of the former than of the latter, that the mode of treatment in the former was inferior to that of the second? We ought to make this deduction, because in so large a number of patients similar circumstances must necessarily be found in both classes; and hence every thing being equal on both sides, excepting the treatment, the conclusion would be rigorously exact. It was in this manner that an investigation was made in regard to cholera; and no one, save the principal person[1] interested in opposing the method, esteemed the plan as a bad one. I would ask how, in fact, it would be possible to know any thing about this subject, unless we counted?

I would likewise observe that the same objection which has been made to the numerical method, viz., that it is difficult or impossible to make groups of perfectly similar cases, might be made to every method which authors would substitute for it; *for it is precisely because it is impossible to appreciate with mathematical exactness any particular case, that we are compelled to count; for in this way errors which are inevitable would be canceled, being found in each group of patients treated in different modes, and therefore they may be neglected without difficulty.*

One of the causes, it is said, which prevent cases thus brought together from being exactly alike, is the difficulty of deciding upon the precise time of commencement of the disease, and the impossibility of deducing the severity, or the period of the disease, from its duration. In answer to this objection, I might refer the reader to the previous reflection; but it will be better to answer it more

[1] I presume reference is here made to M. Broussais.—*H. I. B.*

directly in a few words. Doubtless, it is very difficult to fix upon the exact time at which the disease commences; and no one, perhaps, has insisted upon this fact more than I have done; however, this is by no means impossible, either in acute or chronic diseases, except the subject have but little intelligence, or weak memory, and whose history may, on that account, be considered as, in many respects, of no use for science. As to the impossibility of judging of the severity of a disease by its length, I agree that it is true; but who has ever pretended that these two things were the same, and proportioned to each other? To measure the severity of a disease, have we not the violence of the febrile excitement, the pain, the depression of strength, and certain symptoms which are peculiar to each disease;—in pneumonia, for example, the dyspnœa, and the results of auscultation and percussion?

I would add, that it is much more difficult to fix the exact termination of a disease than its commencement; but, nevertheless, it is very necessary to do so, whatever method we may use, for the just appreciation of the effect of remedies. This is true, even if we reject all method, and confine ourselves to the vague and uncertain interpretation—for it cannot be otherwise—of isolated facts.

It has been stated, moreover, in reference to evacuation of blood, that the employment of leeches cannot, any more than other remedies, be examined absolutely; for whether applied at the commencement, during the progress, or at the termination of a pneumonia, for example—whether the disease be slight or severe, the loss of blood copious or moderate—we cannot deduce any result in relation to its useful or hurtful effects, unless we have mentioned precisely the motives which have caused us to have recourse to them, and have clearly stated the circumstances relative to their application.

If by *motives*, we are to understand that no remedy should be made use of until we have decided that the patient is in a situation similar to those in which it has already been used with success, I understand the meaning of the word, and agree to it fully; inasmuch as it is nothing more than experience applied to therapeutics. But if we are to understand by motives—what we should by indications—*a priori* considerations, I should consider this view as being entirely hypothetical; it counts in what is called rational medicine, experimental medicine (*médecine d'essai*), to which we ought not to refer, except when we cannot obtain any thing better—when we have been taught nothing by experience. Against it, I would contend with all my powers.

The bases upon which I think it possible to place the value of therapeutic means have appeared to some so very ruinous, that they have been astonished at the excessive confidence with which they have inspired me; and it has been thought that I should have avoided error if I had endeavoured, before advancing such opinions, to understand thoroughly the science of numbers. What is calculation, it has been said, but an instrument by which we efface all the differences between the objects to which we apply it, in order

to transform them into absolute and abstract quantities?[1] In answer to this assertion I would state, that calculation, employed as I have described, does not destroy any differences, but that it supposes that they do exist; that it limits itself to the bringing together of similar unities, in order to compare them with others that are similar, but which have been under different influences; and, after all, that if, as we have already stated, it necessarily happens that facts are sometimes brought together whose resemblance is not exact, yet, as the error is in all the different groups, every thing is equalised, and a comparison may be made between different groups of facts, without the truth of the results being altered thereby.

Finally, it is by results that we must judge of the value of different methods. From the earliest ages, the study of therapeutics has been carried on, and therapeutics are yet in their infancy. There is something more necessary to be done than all that has been done heretofore. And as able men have never been wanting in our science, we must refer our present ignorance to the method, or rather to the want of method, with which we have previously pursued investigations on this subject. If we will but render an exact account of facts, and I have already said that there is but one means of doing this, therapeutics will soon make progress, and will present not less certainty than other parts of our science.

But, until the present, there have been so many fluctuations in medicine, observation has been generally so imperfect, what are called its results so variable, so often proved by facts to be false; we are so little accustomed to find that experience justifies what

[1] This and the preceding objections have been urged again, of late, by a physician whose name I will not mention, for fear of seeming to be revenging myself in a mode which is very far from my intention. This physician says:—"By calling to our aid the strictness of arithmetical calculation, in order to flee ourselves from the encroachments of the imagination, we commit the gravest error possible against common sense. It is as if we were to add together rivers, houses, and birds, and from the extravagant sum total thus obtained, we should subtract the fishes and fruits ! ! !" That is to say, if we bring one severe case of pneumonia into comparison with another equally severe, both occurring in subjects apparently similarly situated, though they may in reality differ somewhat, it is like comparing a house with a river! To what class of readers does this author address himself?

After this objection, there come two more which refute themselves.

Moreover, that the numerical method should have enemies, is very simple and inevitable; for, with the exception of axioms, what propositions are there in which all men agree? Happily, for the future well-being of medical science, the numerical method is considered as the sole means of arriving, in medicine, at rigorous and accurate results; and this opinion is held by experienced men—by those whom daily observation enables to decide upon the best method—and the attacks which are made upon that which we pursue will do no injury to it. Its opponents cannot produce any thing objectionable to it, except that it requires long labour. The repugnance to it, on this account, will be overcome by some hard working individuals, and science will advance. I would remark, also, that the utility of the numerical method could not be well demonstrated, except by the objections of its opponents; and that they really are labouring for its advancement.

we read in books, that it will be said that this science, which I make so firm and sure by numbers, will forsake the physician while at the bedside of his patient. Doubtless, science will forsake a physician at the bedside of his patient, if he makes a bad application of it; but how can it be so, if he employs it with discretion? science—*true science*, I mean—being only the summary of individual facts.

The objections, then, which have been urged against the numerical method, when applied to therapeutics, are without foundation. Therapeutics cannot make any greater advances than pathology without it; and to say that this method is not necessary for sure progress in this study, is to deny the necessity of grouping together facts according to their resemblance, and afterwards of numbering them, in order to understand therapeutic agencies. In this assertion, we likewise show a great degree of prejudice, and we forget what we do every day. In fact, when physicians are called to visit a patient, after having come to a conclusion in regard to the nature and character of the affection, they come to the treatment, and if one of them do not have the same opinion that his companions have, in reference to the utility of the means proposed for use, what does he do in order to make his opinion prevail? He does not depend (I speak now of experienced physicians) upon theoretical considerations—upon arguments, *a priori*, which would never persuade any one. He urges his preference for the remedies he would employ, by the fact that he has seen them more frequently followed by success than those proposed by his brethren. In other words, he argues as if he had counted,—without, however, having done it, I allow; and his mode of enquiry is a tacit acknowledgment, or proof, that one cannot fix upon the value of a remedy, except by seeking to determine whether, under certain similar circumstances, it has not met more frequently with success than any other method has done.

It may, perhaps, be said that though this method can demonstrate that a certain remedy is generally better than another, it does not explain how a person—ill with pneumonia, for example, and treated in the same way as a neighbour in exactly the same circumstances apparently—is cured much more rapidly than his companion. I answer to this objection, that the acknowledged advantage of the numerical method is very great, and cannot be obtained in any other way; that when patients, who are thought to be in exactly similar circumstances, get cured in very unequal spaces of time, although treated in the same way, the fact proves necessarily that the resemblance which we thought existed, was not so exact as we supposed. Hence comes a new reason for studying patients with great care, in order to be able to decide carefully the differences and resemblances which they present. But in order to know whether these differences are really of so much importance as one would be ready to attribute to them—whether they really have a marked influence upon the action of remedies—whether they explain the differences observed in the duration of

the disease, it is evident that, on one side, we must put all the cases in which the differences exist which were not at first perceived—on the other, those in which they do not exist; we must count both, and if the durations of the disease in each patient of one class present less differences than those observed previously, we must add these durations, take their mean, and compare it with the mean of the opposite class. In other words, we must count again. Until this is done, or until similar facts are collected and counted, we can hardly be said to have even probabilities in favour of any opinion.

It is scarcely necessary to remark, that the preceding considerations of this and the previous chapter, apply to every thing concerning *external pathology*, any of the means used in treatment. M. Maunoir's work on Cataract[1] sufficiently proves this fact, and doubtless there are but very few surgeons who think that any mode of operation can be considered as proved to be superior to another, without the use of well-authenticated statistics; and it is by this means likewise that we may be able to decide whether any operations which have met with ill success should be continued or not in actual practice. Finally, *anatomy* itself should be studied in the same way, or according to the same principles; for how can we know what to think of the importance of certain varieties in organs, for example, if we do not know the proportion of times in which they occur, if we have not counted? The case is the same with the dimensions of the different viscera and canals of the economy, which vary according to age, &c., and upon which we cannot have very accurate ideas, except by the union of analogous facts which are well authenticated, and by proceeding as M. Bizot has done in his Memoir upon the Heart and Arteries. That is to say, that the study of man, under whatever point of view we regard it, cannot make any progress except we make use *judiciously* of the numerical method. I would add, that in consequence of the fatal effects which frequently follow bloody operations, the rigorous appreciation of the effects of remedies is, to a certain point, still more necesssary in surgery than in therapeutics.[2]

[1] To be translated in a future number.—*H. I. B.*

[2] A discussion lately arose in the Academy of Sciences, between Messrs. Double and Navier, in relation to this method, and as the reader probably wishes to know all that may be said upon this subject, I will here give the remarks of M. Navier, as they were communicated to one of the members, and by him given to me.

Remarks upon the report made to the academy at the meeting of October 5th, 1835, *relative to the "Statistical Investigations" made by Dr. Civiale.*

The perusal of this report, which has been listened to with so much interest by all the members of the academy, has suggested to me a few remarks, which I beg leave now to present to you, while I crave your indulgence in regard to them.

The able writer of this report has treated of an important question, the substance of which is as follows. Is it, in general, useful to apply statistics to medicine; or more particularly, is it useful to collect and record, method-

ically, all observations, to classify, compare, and count them? Can we, from such a method of investigation, deduce results which will contribute to the advancement of science and of art, and guide us in the applications which we must make of them?

It seems to me that this question has been decided in the negative, and that it has been stated as a principal reason for this decision, that *statistics are in fact nothing more than the application of the doctrine of probabilities*. It has been added, that the peculiar province of statistics is to give results which will be applicable to the whole human race, whereas in *practical medicine the problem is always an individual case; that the physician has only one man to treat at each time*, and that general results cannot be applied to individual cases without our being liable to fall into numerous errors. It has been stated, that as the great majority of cases are observed by persons who never write, this majority, of course, is not enumerated. Finally, the system has been opposed by the fact of the infinite diversity of circumstances which the cases present, and for this reason it is said that they cannot enter within the scope of calculation, and that we cannot submit them to an investigation founded upon rigorous and precise methods.

If there exists, in reference to the present subject, any doubt in the mind of any individual, it must, as it seems to me, depend only upon this application of calculation to medical researches founded upon the results of statistics. Some are afraid of this application, because the questions differ very much from those which arise in natural philosophy, to which it is well known that the calculus has been applied with great advantage. It is therefore of importance to have a precise notion of the use which may be made of calculations in researches of this nature, and of the part which we may allow it to hold in the establishment of results to which we would arrive.

It would be difficult to throw doubt upon the utility of observation, the most solid foundation upon which medicine as well as all other sciences can rest. No one likewise can deny, that if these observations are made with care, and recorded methodically, classified accurately, so that those presenting analogous circumstances are brought together, that if they are compared and discussed without prejudice and partiality, no one will deny, I say, that under these circumstances useful results will be deduced. Few persons can fail to regret, that in memoirs on medical subjects, the author seems but too frequently to have for his object, not to examine a question, but to defend a thesis, and instead of presenting to the reader's mind all the data bearing upon the subject, he strives to report those only which are favourable to the opinion he wishes to sustain, and passes over in silence those which are contrary to it. Every one will allow that a different course can alone lead to the discovery of truth, and produce conviction. It will be allowed also that a wholly impartial observation and discussion of facts that are known and have been collected, are elements absolutely indispensable to the true progress of the most useful of all sciences.

But to apply the measures of statistics to medicine, would be not merely to collect observations, for no one can doubt of their utility, but we must connect, arrange, and methodically classify the facts; especially must we count them, and finally present the results of this enumeration as the foundations of certain propositions which will belong to science, and which will lead to the proper application of them. It is this new mode of investigation which seems to have met with opposition.

We should observe that, as yet, we have made no mention of calculation. As it has been justly remarked, common sense is sufficient to enable us to deduce from the enumeration of facts the consequences flowing therefrom. If it has been observed that we are able to save eight patients out of ten by means of an operation,—of course, without any farther calculation, we should infer that it is better commonly to submit to such operation rather than to expose oneself to certain death by not seeking a cure by means of it. What we may expect from the doctrine of probabilities is, that it will give

a measure of the degree of confidence to be granted to the conclusions which flow from the enumeration of the observed facts. Thus, for instance, in the case above stated, common sense shows that if the conclusion, that an operation saves eight out of ten patients, is deduced from only a small number of facts, it becomes of little weight, whilst a result is always more sure, according as it depends upon a greater number of observations—and upon observations, the circumstances of which differ less from each other. But this view, however just it may be, leaves in the mind a degree of uncertainty which the application of the doctrine of probabilities will in a great measure dissipate. By means of this, we shall be able to appreciate very accurately—according to the number and variety of the observations—the degree of confidence which it will be reasonable to give to the consequences which common sense may deduce from them, but upon which some vagueness will always rest. If we use the precise expressions belonging to calculation, we shall know the degree of probability, in case we conform to the general indication which we have obtained, that the result will be such as we desire.

It will therefore be seen that we should err very much if we should reject results, because obtained from enumeration, merely upon the idea that calculation cannot be applied to such investigations. No; the results are not obtained in this case from calculation. They are immediately founded upon observation, which is the purest source of all scientific truths; and they are deduced from it by natural logic, of which calculation never is, and never can be, but the instrument. The use of this instrument presents itself here merely when it becomes necessary; that is to say, when it is necessary to give to consequences a degree of precision to which we never could attain by mere reasoning, and for which the aid of mathematical analysis is really indispensable.

These remarks show very evidently that the application of the calculus differs very much in this case from its use in the physical sciences. In these, the attentive observation of facts has enabled us to deduce certain simple and general laws by which they are governed. The consequences of these laws are developed by reasoning, aided by calculation; and we can thus foresee natural effects, and submit them to a process of investigation, the results of which experience will decide. This scientific method has not yet been applied to medical questions, and perhaps it will never be, because the extreme complication of the phenomena, and the impossibility of repeating experiments as we may wish, prevent us from bringing them to their simple elements—from discovering the natural laws upon which they depend. Thus medicine becomes for us a science of observation chiefly. And by this expression, I mean that we must treat of it specially and solely in the way in which all sciences, even astronomy, have commenced, and which consists in observing facts first in a general manner, afterwards in more detail; that is to say, by distinguishing, by more precision and greater detail, the circumstances with which they are accompanied. Science can and ought to be produced by records, methodical classification, impartial comparison, and enumeration of the facts which have been observed. This method seems likewise to be the only one which is proper for sciences which regulate the public administration and government of society. The results are immediately given by observation. They are necessarily general, as has been remarked, and being deduced from the collection of cases, they do not absolutely coincide with particular facts. But the happy invention of the doctrine of probabilities gives to this mode of investigation all that it previously wanted, by showing us with what degree of confidence these results may be admitted in their applications. Besides, this useful, and, we may say, absolutely essential addition, presupposes the methodical enregistering and enumeration of facts and observations, or what we mean by the term statistics, and I think, therefore, that the use of statistics is eminently useful in medicine.

We might insist, likewise, and admit with little difficulty, that the application of calculation, even when reduced to the use which we are here

describing, that is, to the determination of the degree of probabilities of results, ought, in questions of this kind, to inspire us with entire confidence. We would remark, upon this point, that no one could possibly refuse to admit the results of calculation, except we judge that the elements of the question are too numerous, too diverse, and too variable for calculation, to be able to embrace all with the necessary exactness and justice. But we must remember that in nearly all the cases in which the calculus is employed, and in which the utility of the results of its application cannot be contested, the questions are likewise much too complex to allow of all their elements being taken into consideration. The art of the geometrician consists peculiarly in distinguishing the principal elements, and forming all abstract questions as nearly resembling as possible the natural question, and to which analytical method may be applied. Numberless examples show that solutions obtained in this manner, although differing in some points from natural effects, throw considerable light upon the questions, and lead always to useful results. To mention one merely of these examples, I would allude to the investigations in reference to the motions of sonorous cords. The known solutions present a result which is characteristic and essential, yet very different from natural phenomena, since they show that the vibrations excited in a cord do not grow weaker in time, but that they perpetuate themselves indefinitely, whilst we, in fact, do see the motions of the cord diminish gradually, and cease wholly in a comparatively short space of time. Nevertheless, neglecting this discrepancy, which arises from our not regarding the fact that a motion is excited by the cord in the air, and the part to which the cord is attached, still this investigation is very interesting, because it establishes precisely what is confirmed by observation, viz., the relations existing between the weight of cords, their length, degree of tension, and the sounds emitted by them. And these relations are the chief objects of the question. The application of the doctrine of probabilities to the subject now under consideration, presents analogous circumstances, and as much utility. In each particular case we should form an abstract question as nearly as possible similar to the natural one, and to this calculation will apply strictly, and though the solution may be generally different from the solution which would follow the true question, it would shed, however, much light upon this last. After such a solution, the mind will be in a very different state from that into which it would have fallen, had we made no examination whatsoever.

It does not seem to me that the fact that practical medicine is always an individual question, and that the physician has never but one person to treat, are sufficient objections to make us reject the method of examination which consists in deducing general results from a collection of facts. For it appears to me that no physician ever pretends to solve this individual question except by referring to observations made by himself or others who have preceded him, and upon whose instructions he depends. All his science wholly disappears before a case which is to him wholly unknown. He applies, then, his principles, which are the results of general observation. He applies them with the more confidence, according as the disease may be more or less common, and more or less frequently observed. Doubtless, the general results of common observation may be contradicted in one particular case. This circumstance depends upon the nature of the science, which will not allow of any absolute rules. The mathematical sciences are probably the only ones which are capable of allowing of them, for they are founded on purely abstract definitions. In proposing to apply statistical methods to medical investigations, no one pretends that the results will be like geometrical theorems. It is contended merely that by studying cases more in mass, with more impartiality and with greater method, and by giving more exactness to the consequences deduced from them, this science will make more rapid progress, and will be applied in a more certain manner.

Doubtless, as it has been already observed, tables of mortality teach nothing certain in reference to the moment at which a certain man is to die, but it is

not the less true that these tables afford us information of the most valuable character in an infinite number of cases. A remedy commonly cures, but at times it may wholly fail, in consequence of causes which we cannot perceive. The physician will prescribe the remedy, though he is not entirely certain of success; the geometrician, after having counted the observations previously made, will likewise indicate the degree of probability of cure.

It has been judiciously remarked, that as the majority of physicians do not write, the majority of the facts which fall under their notice escape any investigation, and consequently are forgotten. But it by no means follows from this, that we cannot deduce useful consequences from observations which have been well made and carefully recorded. As natural phenomena are often dependent upon general laws, though they may be unknown, it is not indispensably necessary that we should have all the facts in order to acquire a knowledge of the results of these laws. Doubtless, the conclusions to which we should be led would be the more certain in proportion to the number of the observations upon which they are founded, and this is a greater reason for multiplying and recording them with care. But even when the observations are few in number, if they have been recorded and collected without bias, useful results may be deduced from them, which may be used with much advantage. It is in this case that the doctrine of probabilities comes to our aid, and will enable us to fix our ideas upon the subject, and by the comparison of the number of facts observed with the number not observed, we may be led to judge what degree of confidence ought to be given to the results. This method will tend to give daily more exact and precise notions upon subjects in relation to which we have been for so long a period wholly in the dark, and from which we never shall escape, whilst from an ill founded distrust in the use of calculations, we continue to reject the only scientific method which can fully dissipate this uncertainty.

The complication and diversity of natural effects, the variety of accidental circumstances, the obscurity and multiplicity of the laws of all vital phenomena, the impossibility of reproducing at will any of the facts which we should observe in medicine,—these reasons ought not, as it seems to me, to make us wholly reject this method upon which we are now insisting. These circumstances form, in fact, the character of those sciences which, in my opinion, cannot be treated except by observation ; hence, as it seems to me, it results that we ought to apply, as speedily and as generally as possible, this method; for we see that in order to arrive at sufficiently precise results, we must multiply the number of our observations and collect carefully all their elements and details, and classify them prudently yet rigidly. The extent and difficulty of this task ought not to prevent us from attempting it, if truth is to be our reward.

I hope the Academy will pardon these brief remarks, which I submit chiefly to those persons whose opinions I have not been able wholly to coincide with, notwithstanding they were imparted with talent. I would simply draw this conclusion, that the application of statistical methods to the study of medicine, and the use of the doctrine of probabilities, which is the complement of it, seems to be the surest means of giving to medical studies a truly scientific character, and all the rigorousness and precision of which they are capable.

CHAPTER III.

METHOD OF ANALYSING FACTS.

It is not sufficient to have laid down, in a general way, the method we must follow in order to rise from particular facts to the laws which govern them; but we must enter into some detail in regard to this subject, and see what methods we ought to pursue in order to attain the end in view.

The reader will remember, what has been stated above, that in order to form a just idea of a symptom or of a lesion, in any disease whatever, we must study it in all the subjects of whom we have full histories. The more numerous these cases are, the more exact will be the results to which they will lead, and moreover the more impossible it will be to trust them, even for a moment, to the memory, in order that we may make an analysis of them. To gain our end we must make use of mechanical means, as it were, or, in other words, decompose each fact, so as to be able to view its various parts with care. And first let us suppose that the question is in relation to symptoms, and that the facts to be analysed are a hundred in number.

The first thing to be done is to change the form of each observation, to divide it into as many distinct sections as it contains distinct objects without suppressing any thing which is in the original observation, preserving exactly the different dates. Thus, for example, as every thing relative to the tongue during the course of an acute disease is altered about in as many notes or portions as there are days on which the examinations of the patient have been recorded, we should place every thing relating to that organ in the same column with the day of the disease in which the record was made. As much should be done with reference to the throat, stomach, (nausea, vomiting, pains in the epigastrium,) abdomen, respiration, circulation, &c.

And as it may be possible that in transcribing we may commit some error or make some omission, we must be able always to refer to the original paper, in order to verify the copy, &c. &c.; we should put at the head of the copy and of the original observation, a certain number, &c. In a word, we should decompose an observation in the following manner:

NO. I.—SYMPTOMS OF COLICA PICTONUM.

(Extract from a table in which are to be found all the symptoms experienced by a hundred patients affected with lead colic.)

Number.	Sex.	Age.	Residence at hospital.	Residence at Paris.	Tongue.	Thirst.	Appetite.	Epigastrium.	Nausea.	Vomiting.	Dejections and Urine.	Abdomen.	Pulse.	Skin temperature.	Perspiration.	Intelligence, Sleep.	Physiognomy, Strength, &c.	Senses.	Treatment.	Causes.
8	m	19	11 d.	7 y.	16, colour of coffee mixed with milk; 17, ibid: 19, " 21, a little gray at centre: 27, ibid.*	5 to 15, none: 17, slight.	5, good: much less afterwards: 17, a little: 21, good.	16, not painful except when colics: 27, digestion easy.	5 to 15, none except when vomiting: 16, 17, 21, ib.	1 to 4, none: 5, immediately after supper: 15, after nausea and emetic.	5 to 15, some by enemata: 16, costive: 17, five liquid stools: 21, six: 27, regular.	5 to 15, colics at times: 16, ib., abdomen good shape: 17, colics much less: 21, no colics since 17: 27, ibid.	16, sufficiently large, rich, hard, 62.	No icterus: 16, heat: 17, no sweat in night.	Short colds in winter: 5 to 15, a little cough, less latter days: 16, cough rare, no rale.	1 to 15, no headache: 5 to 16, little sleep, because of pain: 17, sleep moderate: 21, good, without headache: 27, ibid.	6 to 15, repose, no pain in limbs: 16, face natural, no pain in limbs: 17, fatigue: 21 to 28, no pain in limbs, return of strength.	1 to 5, coryza.	14, manna, oz. ij. with ant. tart. gr. i.: 16, cassia water with ant. tart.; theriacum with opium, gr. i.: 17, purgative enema theriacum with opium, gr. i.: purge, 18: ib. 19: 25, 3-4 loaf.	Plumber 3 past years; weighs and unloads the lead. Not sick for 7 years before. Some excess in drink'g wine from 16 yrs. age.
9	m	29	13 d.	3 y.	9 to 15, mouth pasty, bitter: 16, tong. clean: 17, ibid: 20, a little yellow, not coated. mouth with bad taste, not pasty: 27, tongue natural.	4 to 5, thirst at night: 16, 20, 23, 24, none.	1 to 15, anorexia: 16, ibid, with loathing of food: 20, appetite great at sight of food: 23, anorexia: 24, appetite: 27, enormous.	16, not pained by pressure.	Nausea before 9, none after: 17, none: 20, 21, ibid.	Vomit'g of bile after emetic: 17, six vomitings: 18, ib. without bile: 20, vomiting of purge 2 hours after taking it.	Frequently costive: from 9 to 15, costive: 17, enema returned without fæces: 18, two dej. after enema without hard fæces: 19, no dej: 20, urine scalding: 22, no dej: 23, six dej.: 24, enema returned without any thing else	9 to 15, colics increasing: 16, pains constant, heavy, below umbilicus augmented by pressure: 17, colics: 18, none: 19, ib.: 20, colics relieved by pressure: 22, increas'd during night: 23, less: 24, during ngt.: 27, none.	16, at 40: 17, ib. during emetic: 20, at 49 sufficiently large: 23, large, full, slow: 24, ib.	1 to 15, daily chills evening, without increased heat, generally: 17, chill a little before usual hour: 18, no fever: 20, 22, ibid: 23, heat in abdomen, especially at night.	Colds but seldom, never pneumonia nor pleurisy: 18, 20, no cough nor rale	15, headache 1st time: 17, a little more: 18, none, calm sleep: 20, watching, because of pain: 21, worse than before: 22, restless, without headache: 23, better, no headache: 24, much uneasiness, without fever: 27, sleep excellent.	1 to 15, pain, no cramps: 16, none in limbs, some in loins during fever: 17, no pain in limbs: 18, pain chiefly in loins: 22, restless: 23, considerable debility: 27, no pain in limbs.	No troubles in eyes in childhood.	10 to 15, no food: 9 tart. ant. grs. vj.: 16, cassia water with tart. ant: 17, sol. of tart. aut.: 18 purge for next day: 20, ibid with mint water and syrup of poppies: 22, castor oil, oz. ij.: 23, theriacum with opium gr. i.: 24, 1-4 house allowance.	House painter 2 years: cabinet maker before.

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* The numbers in this and the following tables indicate the day of the disease that the symptoms were recorded.

But it is evident that if this table contains a hundred observations of the same kind as it has been supposed, or even a much smaller number, for example, fifty, thirty, fifteen, even, it would be impossible to analyse any one of these columns without subdividing it, or, at least, before having analysed each fragment of it. This last method will be sufficient for those which have few points to be noticed, or which do not present a great variety of objects; as, for instance, those relating to the tongue, appetite, epigastrium, &c. &c. We must divide them, when the contrary is the case, as for the abdomen, &c. &c. This might afford five secondary tables, which may be marked by the numbers 2, 3, 4, 5, and 6, and upon which it would be useless to enter into more detail.

NO. II.

No.	Sex.	Duration.	Tongue.	Throat, Thirst.	Epigastrium.	Nausea.	Vomiting.
8	M.	27 d.	16 to 19, a little of colour of coffee mixed with milk; 21, grayish.	5 to 15, no thirst; 17 slight.	16, not pained by pressure, except during colics.	15 to 21, none, save before vomiting.	15, after supper, none before; 16, after emetic.
9	M.	27 d.	16, 17, 27, natural. 20, yellowish.	4 to 15, thirst during the night; none afterwards.	16, not pained by pressure.	Before 9, none afterwards.	9, 17, 18, 20, after emetic or purge.

NO. III.

No.	Sex.	Dur.	Dejections.	Form of Abdomen.	Pains in Abdomen.
8	M.	27 d.	5 to 15, some by enemata; 17 and 21, five and six, liquid.	16, natural.	15, 16, colics, variable; 17, much less; none afterwards.
9	M.	27 d.	9 to 15, constipation; 18, two dejections by enemata; 23, six; not frequent before.		

NO. IV.

No.	Sex	Dur.	Pulse.	Chills.	Heat.	Sweat.	Skin.
8	M.	27 d.	16, at 62.		16, during night.		No leterus.
9	M.	27 d.	16, 17, at 40; 20, at 49.	15 to 17, daily, at evening.	23, internally; none previously.		

NO. V.

No.	Sex.	Dur.	Cough.	Expectoration.	Rale.	Pain in Side.
8	M.	27 d.	5 to 16, decreasing during latter days.	Colds short during winter.	16, none.	
9	M.	27 d.	18 to 20, none.	Colds rare; no pneumonia, nor pleurisy.	15 to 20, none.	

NO. VI.

No.	Sex.	Dur.	Head.	Intelligence, Sleep.	Limbs.	Strength.	Senses.
8	M.	27 d.	1 to 15, 21, 27, no headache.	5 to 16, no sleep, because of pain; sleep afterwards.	6 to 15, 21, 28, no pains; 17, fatigue.		1 to 5, coryza.
9	M.	27 d.	16, 17, increasing headache; none afterwards.	20, little sleep, on account of pain; more sleep afterwards.	1 to 15, pains without cramp; none afterwards.	23, considerable debility.	5, no disease of eyes during childhood.

When this has been done, the analysis of the cases becomes possible, and has not, in fact, many great difficulties. We can very soon see, by running through one of the columns of these last tables, how many times one symptom has occurred in a certain number of persons, at what period it commenced, when it ceased, how often it was slight, and how often severe. And if afterwards we wish to compare the symptoms with the lesions in subjects who died, this may be done with equal facility, by reference to the number which ought to be the same in the tables relating to the symptoms and those containing the lesions.

As two of these small columns relate to the age and sex, we may, by means of still smaller tables, learn whether these two facts had any influence upon the course of the symptoms, their duration, &c. &c.

The method we should pursue in the study of lesions is the same as that we have just described for the symptoms. The part of the observation relating to the condition of the viscera must be changed, and placed in tables, so that every thing relating to one organ may be in one column, as in the following table, which is the commencement of a large one in relation to the typhoid affection.

NO. VII.—TABLE OF ANATOMICAL LESIONS OF THE TYPHOID AFFECTION.

(Extract from a table in which are described the lesions observed in fifty-five patients who died of the typhoid disease.)

No.	Sex.	Age.	Dur.	Stomach.	Small Intestine.	Colon.	Mesentery.	Liver.	Spleen.	Lungs.	Larynx.	Heart.	Brain.	
1	M.	19 y.	30 d.	Medium size; numerous folds along great curvature; mucous membrane generally grayish, covered almost wholly with a viscid mucus of considerable thickness; moderate consistence; as much in great cul-de-sac as along great curve; rather reddish in its substance; not much elevated.	Generally and moderately distended; containing in its first half a large quantity of mucus, less in second; mucous membrane generally of a grayish hue, of natural consistence and thickness, and in its second half are many patches, covered with gray points, but not thickened, but in the last five feet patches thickened, like a cake; prominent; prominence owing to mucous & cellular coats composing part of patch; both red; half line thick, softened; the last five patches partially ulcerated; four lymphatic glands between submucous and muscular coats of cœcum.	Moderately dilated, containing a pultaceous clear greenish substance; mucous membrane soft in cœcum, and in the ascending colon; very healthy elsewhere; in cœcum and ascending colon, many ulcers with flattened edges, gray and blackish circumference, and twenty tumours, from half to two lines thick, composed of cellular membrane changed into a whitish substance, streaked with vessels covered with mucous membrane, which is partly destroyed in this part.	Glands very much larger than usual; of an obscure red, moderately deep colour; soft, but not in a putrid condition; right mesocolic and meso-cœcal glands in same condition.	Natural colour, size, and consistence; gall-bladder moderately distended, with a thin deep yellow bile.	Of usual size, colour, and consistence.	Left lung free; some adhesion of the right; both generally red; the left with some black spots of an inch or more in surface, corresponding to hepatised masses; or which are hard, but not granulated; this was the condition of a great part of the base and posterior part of the right lung, which presented no traces of hepatisation.	Healthy; trachea red below; bronchi more so, of a dull red colour; mucous membrane perfectly well otherwise.	Of a good size; its left ventricle very firm, from six to seven lines thick; aorta healthy, being twenty-nine, nineteen, sixteen, twelve lines in size; pulmonary artery twenty-nine lines.	Effusion under arachnoid very slight; depression an inch in surface, four lines deep upon the sides of the left lateral lobe; filled with a certain quantity of semi-cortical substance of a rosy hue; whole mass of brain of a good consistence; a spoonful of serous fluid in each lateral ventricle; another at the base.	On the right fore arm, in consequence of erysipelas, effusion of pus, similar to cream, of a rose colour, between skin and muscles, and between deep and superficial muscles; moderate emaciation.

We next analyse each of these columns, as we have said, with respect to the symptoms, by subdividing it into as many secondary columns as it contains distinct objects: thus, for example, with relation to the stomach we should place in as many particular columns what relate to the consistence and thickness of its mucous membrane, its ulcerations, its state with regard to enammillation, its softening, with redness or without alteration of colour, its healthy state, or its variation from it, by slight change of colour. If the enquiry be as to the small intestine, we should also place in as many secondary columns whatever relates to the size of the organ, the matter it contains, the colour of its mucous membrane, its consistence and thickness, the state of Peyer's patches, of Brunner's glands, and of the mesenteric glands corresponding to the altered or unaltered patches.

The only means of giving an account of numerous facts, of accurately analysing, and studying them in all points of view, evidently is to decompose each observation as I have just stated. We could render more perfect the tables, whose outlines I have given, by increasing their divisions, &c., according to the number and importance of the objects studied in each observation, but it is absolutely necessary that they should be put into practice; and if I decide so confidently in favour of this method, it is because, having employed it in all my own researches, I have obtained no result which experience has not confirmed. Experience itself has thus decided the question.

Suppose this previous labour to be done for a great number of diseases, science will not yet exist, but will be near its completion, and but little labour will be required to bring it to a point far superior to its present one. Suppose we wished, as I have just shown, to know some point in the history of a disease—for example, whether delirium is more frequent in the course of pneumonia than in that of other acute affections—we shall arrive almost immediately at the knowledge of the fact by running through the columns relating to cerebral symptoms in all the affections, which we ought to examine on this point. If we afterwards wish to know precisely when this symptom commences, when it terminates, whether it varies with the age, sex, strength of individuals, &c., we shall experience no difficulty. The same is the case if we wish to compare the symptoms with the state of the organs in patients who died; so that as soon as an idea presents itself to the mind, we might verify it by placing it in relation to facts, and thus, perhaps, we shall never see the most probable proposition realised; but we shall always discover some fact, usually of much more interest than the one we conceived; at least this has been constantly the case with myself, when I have formed a previous idea of the manner in which a fact must happen, and have endeavoured to find by analysis what really had occurred.

A TREATISE

ON THE

CHEMICAL, MEDICINAL, AND PHYSIOLOGICAL PROPERTIES

OF

CREOSOTE,[1]

BY JOHN ROSE CORMACK,
Member of the Royal Medical and Royal Physical Societies of Edinburgh.

My taste for dead bodies and every thing like MUMMY is decided.—CARATHIS IN VATHEK.

PREFACE.

From the recent discovery of creosote, many interesting facts connected with its history are probably still undiscovered, and unquestionably very much remains to be done in the investigation of its medicinal properties, before we can form a proper estimate of its real value as a therapeutic agent. An attempt has been made in the following essay, to collect all the information upon the subject which has been published in the Foreign and British journals, and, at the same time, to state such further details and observations as were known to the author.

Before describing the chemical and physical properties of creosote, it has seemed proper to give an account of the products of the destructive distillation of vegetable matter, and more particularly of those proximate principles which were discovered in the tars by Reichenbach. This appeared necessary from their being all generated along with creosote, and because their separation from it requires a variety of chemical manipulations, the *rationale* of which can only be properly understood by those acquainted with the relations which these substances bear to each other and to creosote.

Under the chemical properties of creosote naturally fell the consideration of its antiseptic virtues, and this suggested some remarks upon the embalming of the Egyptians, and the preservative power of peat-moss, from a belief that the Egyptian mummies and the fresh bodies found in peat water owed their preservation to creosote. To have entered fully into the consideration of these interesting topics would have been out of place, the subject having been announced for competition by a strictly medical society, by whom it was supposed any lengthened disquisition on these subjects would have been considered no additional recommendation of the paper which contained it.

The other subjects discussed in this essay are intimately connected with medicine, although apparently but a disproportionately small part is devoted

[1] Illustrated by Experiments on the Lower Animals: with some Considerations on the Embalmment of the Egyptians; being the Harveian Prize Dissertation for 1836. 8 vo. pp. 164. Edinburgh, 1836.

to this subject. It is conceived, however, that considerable light may be thrown upon the therapeutic properties of creosote, by attending to the effects produced by it when administered to the lower animals, and by comparing the ancient medicinal uses of substances containing creosote with those purposes for which the drug itself has been employed in modern times. For although there is much rubbish in the old works on the materia medica, yet the remarkable coincidences in the present instance are by far too striking to be passed over in silence. Notwithstanding that the physiological properties of creosote were made the subject of a number of experiments, the author is aware that the account which he has been able to give of them is, in some respects, imperfect, which must in a great measure be attributed to the numberless inconveniences and difficulties attending such investigations, and from the slender assistance which could be derived from the observations of former experimenters. Should the farther elucidation of this interesting part of the subject not fall into abler hands, it is intended, at some future period, to endeavour to supply these defects, and correct such errors as may have been committed.

2, London Street,
March, 1836.

PART FIRST.

CHAPTER I.

PARAFFINE, EUPIONE, PICAMAR, CAPNOMOR, AND PITTACAL.

The dry distillation of organic substances consists in subjecting them, when deprived of moisture, to an elevated temperature. During this process, the elementary principles of the body operated upon enter into new combinations, so that the products are the result of its destruction or decomposition by heat. Before the heat has been much raised, carbonic acid gas is given off, which is followed by carbonic oxide, water, acetic, and other acids, and lastly come the oily and less volatile products. If the substance contain any nitrogen, part of it unites with some of the hydrogen, to form ammonia, and some of it may also combine with part of the carbon, and produce cyanogen. After a certain period these compounds cease to be generated, and there remains in the retort a quantity of carbon, uncombined with any other substance, which no increase of heat will volatilise.

It was when investigating this curious subject, that M. Reichenbach, the chemist of Blanksko, discovered creosote, and five other new substances, all interesting to chemists, and peculiarly import-

ant, on account of the useful purposes, in medicine or the arts, to which they have been already, or may at some future period, be applied. M. Reichenbach has published accounts of these new substances in Schweigger-Seidels Journal for 1830, and the subsequent years; and it appears proper, as introductory to this essay, to give a short sketch of what is known concerning them, viz., paraffine, eupione, capnomor, picamar, and pittacal, the substances which he found associated with creosote, and all of which are products of the destructive distillation of vegetable matter.

Paraffine is so called from its remarkable want of affinity for other substances, its name being derived from two Latin words, *parum*, and *affinis*. An account of it was first published in August, 1830, by Reichenbach, in the German journals; but Dr. Christison, of this University, discovered it about the same time, and, early in 1831, read an account of it to the Royal Society of Edinburgh, ignorant of its having been previously described.[1] From his having first obtained the substance from the petroleum of Rangoon, he gave it the name of petroline; but as the discovery was first announced by Reichenbach, it has retained the appellation of paraffine which he gave it.

It is a white, hard, crystalline substance, without taste or smell; but at a temperature of 111° F. it is a transparent and colourless liquid, of oleaginous consistence, and at a higher temperature distils unchanged, being volatilised in the form of white vapours. It is a non-conductor of electricity—resists the action of the strongest acids, and alkalies, and of chlorine, and potassium. The density of paraffine, in the solid state, is 870. It is easily dissolved at common temperatures by oil of turpentine, oil of tar and naphtha. With the aid of heat it is soluble also in the fatty oils. It is very sparingly soluble in cold alcohol, and when water is added, the paraffine is precipitated. Boiling alcohol only dissolves by weight three and a half per cent., and on cooling the solution thickens. When fused with camphor, naphthaline, and pitch, no action ensues, but it unites with wax, stearine, and cetine, when treated in this way. The alcoholic solution does not affect the vegetable colours. Paraffine burns without smoke, and equals wax in the whiteness and beauty of its light. It is ignited with much greater difficulty, but has the advantage of being more slowly consumed; and could a tolerably cheap and easy process be devised for procuring it, there is little doubt but that it would soon be in demand for the manufacture of candles, (to grace the saloons of the fashionable,) especially as the petroleum from which it may be obtained is a very abundant natural production in some countries. Reichenbach has also suggested, that it might be used for making bougies, and greasing the wheels of machinery.

M. Jules Gay-Lussac, considers its atomic constitution isomeric with that of olefiant gas, which is composed of one atom of carbon, and two of hydrogen; but as neither the equivalent of paraffine,

[1] Transactions, Royal Society of Edinburgh, vol. xiii. p. 118.

nor the density of its vapour are known, this analysis is by no means satisfactory.

M. Reichenbach discovered *eupione* at the same time as paraffine. Its name is derived from the Greek adverb ευ, and πιων, *greasy*, from its possessing this property. It is an oily substance, destitute of taste, colour, and odour, and is the lightest known liquid; its specific gravity being no more than 655. It retains its liquid form at 4° F.; boils at 339° F.; and distils unchanged, and without leaving any residue. Like paraffine it is unaffected by the strongest acids and alkalies, and like it also burns with a clear light, without smoke or odour. It has also the advantage of being slowly consumed. These properties render it very suitable for lamps. Carbonic acid and water are the sole products of its combustion. It is insoluble both in cold and hot water. Alcohol is its best solvent: 33 parts of absolute alcohol at 63° F. dissolving 100 parts of eupione. With the assistance of heat, these two liquids unite in all proportions. Acetic ether dissolves about a third of its weight of eupione and sulphuret of carbon; and ether, naphtha, spirit of turpentine, oil of almonds, and olive oil, mingle easily with it in the cold. Eupione dissolves camphor, stearine and cetine, cholesterine, naphthaline, and paraffine, the action being much assisted by heat. Iodine is dissolved by it in the cold, with the production of violet vapours; and it also dissolves chlorine and bromine. These three substances are expelled by heat, and the eupione is left unchanged. It dissolves caoutchouc with great rapidity. It is an excellent substance for preserving potassium. It probably contains less carbon than paraffine, but differs from it in no other respect. However, its exact composition is as yet only conjectured.

Eupione is more abundant in the animal than in vegetable tars. In the process of distillation it comes over along with the paraffine, and there is much difficulty experienced in separating them. It is best obtained by distilling the tar of bones. The first product must be repeatedly distilled from strong sulphuric acid, and then washed with an alkaline solution. After this has been done, the eupione remains associated with paraffine only; but to separate the two substances requires the most careful manipulation, unless the mass be subjected to a cold of zero, when the paraffine may be separated by crystalisation. Eupione being more volatile than paraffine, may, with cautious manipulation, be separated from it by distillation from water, and if the impure product has been treated with alcohol, the process succeeds better, as eupione is more soluble than paraffine in this menstruum.

Picamar is the bitter principle of tar, whence it derives its name, (*in pice amarum.*) When applied to the body externally, it does not exert any peculiar influence. It is a thick, oily, colourless liquid, and has a peculiar, but not disagreeable odour. Its specific gravity is 1100. It boils at 545° F. It refracts light, but not so powerfully as creosote. In eupione it is not soluble, and in water very sparingly, but it is dissolved by alcohol, ether, and creosote in all proportions. It has no action on test paper. Sul-

phuric acid dissolves it without decomposition. It is decomposed by nitric acid. When boiled in the atmosphere it acquires a light brown colour. A stream of chlorine produces the like change, thickening it at the same time. Its action with bromine is remarkable. If one part of bromine be added to four of picamar, a deep red brown colour is immediately produced, the temperature rises, and bromine is disengaged in the form of vapour. With iodine it forms a thick liquid mixture. It dissolves sulphur, phosphorus, and selenium. With potassa it forms compounds which are very sparingly soluble in alcohol. Soda, lime, baryta, and ammonia, form with it combinations which require for their solution a hundred parts of water.

This substance exists in the heaviest portions of the tar of oil. In preparing creosote, it unites with the potassa used for purification, forming a crystalline compound, which, after repeated solutions and crystalisations in water, is decomposed by phosphoric acid, and then separated by distillation. To obtain the substance in perfect purity, it is necessary to conduct this last part of the operation *in vacuo*, on account of its tendency to unite with the oxygen of the atmosphere.[1]

Capnomor is so called from the circumstance of its being one of the constituent parts of smoke, its name being derived from two Greek words, καπνος, *smoke*, and μοιρα, *part*. It is a colourless and transparent liquid refracting light nearly as powerfully as creosote. It has an agreeable but not very strong odour, somewhat resembling that of ginger. Its specific gravity is 0.975, and its boiling point 363° F.

It has no action on turnsole or turmeric. It does not absorb oxygen, either when cold or heated. When heated, it yields a good deal of smoke. In cold water it is almost entirely insoluble, but is dissolved in a small quantity of boiling water. It is soluble in alcohol, ether, oil of petroleum, carburet of sulphur, creosote, eupione, and picamar, but is almost insoluble in oxalic acid. With the assistance of heat it dissolves caoutchouc, which in cooling is deposited unchanged. Without heat it causes caoutchouc to swell.

When brought into contact with bromine and chlorine, hydracids and peculiar oily substances are formed, the temperature at the same time becoming elevated. If the capnomor contain the least trace of pittacal, the first bubbles of chlorine produce a violet colour, which passes into yellow after the capnomor has become saturated. In the cold it dissolves iodine, phosphorus, sulphur, and selenium.

Weak nitric acid produces with it a deep brown colour, and in a more concentrated form decomposes it; a new crystalline substance being formed along with some acids. Sulphuric acid of sp. gr. 1.850 dissolves its own weight of capnomor without decomposition, but the temperature soon rises, and sulphurous acid vapours appear.

[1] Journal de Chimie Médicale, et de Toxicologie. October, 1834.

This substance exists in the heavy portions of the oil of tar, and is with difficulty separated from the other bodies therein contained. In the preparation of creosote, a considerable quantity of it is seen floating on the surface of the solution of potassa, when the other ingredients are dissolved. When collected it is combined with a little sulphur, to separate it from which, it ought first to be dissolved in sulphuric acid. The acid is then to be saturated with carbonate of potash, when the capnomor separates and may be obtained quite pure by distillation. It may be obtained from the animal, as well as the vegetable tars, and from the oil of Dippel.[1]

Pittacal is a solid substance, of a fine blue colour, and may be fixed as a dye. Its name is derived from πιττος, pitch, and καλλος, *ornament*. It contains nitrogen, and seems to belong to the same class of bodies as indigo. Like that substance, when rubbed, it acquires a metallic lustre. It is insipid, inodorous, and not volatile, is almost insoluble in water, but is so minutely divisible in that fluid as to pass through the filter along with it. In a few days, however, it collects at the bottom, leaving the water quite colourless. It dissolves in the cold, without decomposition, in diluted sulphuric and hydrochloric acids, but is insoluble in alcohol and ether. Nitric acid decomposes it. It is dissolved very copiously by means of acetic acid. Its acid solution has a red colour, which is converted into blue by the addition of an excess of alkali. Reichenbach states, that it is a more delicate test of acids and alkalies than turnsole. It has no action upon this substance.

With acetate of lead, hydrochlorate of tin, and acetate of alumina, it strikes a beautiful deep blue colour, somewhat resembling that of the violet. When the hydrochlorate of tin, or acetate of alumina is used, the colour is so permanent as to resist the influence of light, soap and water, ammonia or wine.

Reichenbach has not yet furnished us with the process by which he obtained this substance—a circumstance much to be regretted, as no other chemist seems to have procured it. Its presence in the oil of tar, however, can easily be shown by adding (after neutralising the acid) a small quantity of a solution of baryta. A beautiful blue colour is immediately produced, which in about five minutes assumes a blue shade, exactly similar to that of indigo.[2]

CHAPTER II.

PHYSICAL AND CHEMICAL PROPERTIES OF CREOSOTE.

Creosote is by far the most interesting of all the products of

[1] Journal für practischen Chemie. Vol. i. Cacheir, as translated in the Journal de Pharmacie. May, 1835.

[2] Journal de Chimie Médicale, et de Toxicologie. Oct. 1834.

destructive distillation, both on account of its chemical properties, and its numerous useful applications. It was first discovered by Reichenbach in impure pyroligneous acid, and afterwards in all the tars. Its name is derived from two Greek words κρεας, flesh, and σωζω, *I save*. In this part of the essay, its physical and chemical properties, the adulterations to which it is liable, and the best method of preparing it, are to be severally discussed.

Creosote is an oily fluid, transparent, and when pure, perfectly colourless. Its odour is very similar to that imparted to meat by wood smoke, but it is liable to considerable variation, dependent on the particular species of tar from which it has been obtained. It has a burning taste, followed by sweetness. At 68° F. its specific gravity is 1037. It boils at 397° F., and is not congealed at — 17° F. It burns briskly in the atmosphere, emitting a large volume of smoke. When dropped on paper it imparts a stain, but this disappears in the course of a few hours, and much more speedily if a gentle heat be applied. Creosote is a non-conductor of electricity. It unites with water in two different proportions; one of the combinations being a solution of one part of creosote in four hundred parts of water, and the other a solution of ten parts of water in a hundred of creosote. Alcohol, ether, carburet of sulphur, eupione, oil of petroleum, naphtha, and acetic acid, combine with it in all proportions. It has neither an acid nor alkaline reaction with test paper, but it forms a number of interesting compounds, both with the acids and alkalies. Nitric acid acts upon it with violence, giving rise to a copious disengagement of red vapours. Concentrated sulphuric acid, in small quantities, gives it a red colour, but when more of the acid is added, this colour becomes black, and the substance formed is of a more viscid consistence than creosote; the sulphuric acid is decomposed, and some sulphur set free, which may be separated by distillation. It has a strong affinity for all the organic acids, but more especially the acetic, which may be said to be its specific solvent. It combines with chlorine, bromine, iodine, phosphorus, and sulphur. When chlorine unites with it, it first assumes a pale yellow and then a deep reddish yellow colour, and a resinous substance is formed which contains no creosote. Phosphorus and iodine are rapidly dissolved by it. In the cold it acts upon sulphur slightly as a solvent, but with the assistance of heat, about thirty-seven parts are dissolved, and a reddish brown liquid is formed, from which, on cooling, the greater part of the sulphur is deposited in the form of crystals. Concentrated creosote dissolves the oxide of copper, producing a chocolate brown colour. It reduces the oxide of mercury to the metallic state, and a resin is formed wholly destitute of creosote, as was formerly stated to be the case when this substance combines with chlorine. When potassium is thrown into creosote it disappears,—potash is formed, and gas is disengaged. The potash remains combined with the thickened creosote, but may be separated from it without change by distillation. Creosote forms two different combinations with potash, the one anhydrous and oily, and the other hydrated and of a white colour.

The latter crystalises in small pearly spangles. It forms similar compounds with soda. All the acids, even the carbonic, have the power of disengaging it from its combinations with potash and soda. Creosote combines with lime and hydrate of baryta, forming white, unctuous substances, soluble in water; deprived of which, they exist in the form of pale rose-coloured powders.

Ammonia is soluble in creosote at all temperatures. A great number of the earthy and metallic salts are dissolved by creosote: some of them, however, requiring the assistance of heat. In those cases in which heat is necessary, the salts generally separate on cooling in the form of crystals. This takes place with the acetates of soda, potash, ammonia, zinc, and lead, and the hydrochlorates of lead and tin, &c. &c. It decomposes the acetate of copper, and then acts on the acid and base separately, forming a brown-coloured liquid. It reduces the acetate of silver, the metal being precipitated in the form of a white powder, which assumes a metallic lustre under the burnisher. With the assistance of heat it dissolves the nitrate of silver. All the resins and their colouring principles are either decomposed or dissolved by creosote, some of them requiring heat; and there are no organic substances which it attacks so briskly. At a high temperature, it dissolves the colouring matter of indigo, which is precipitated again by alcohol. It forms a yellowish red solution with cochineal, a pale yellow with *santalum citrinum*, [sandal-wood?] a golden yellow with saffron, a yellow with madder, a deep red with dragons' blood, a red with Saunder's wood, and a deep purple with sorrel. With the aid of boiling, it dissolves caoutchouc. It has no affinity for paraffine.

The most important chemical property which creosote possesses is that of coagulating albumen, as on this depends its powerful antiseptic virtue. The action which takes place when albumen is coagulated by creosote is not exactly ascertained, but it may be explained according to the hypothesis of Fourcroy, by supposing that oxygen gas is absorbed. There can be little doubt that the antiseptic power which creosote possesses depends upon its coagulating albumen, for neither is muscular fibre by itself incapable of entering into a state of putrefaction, nor does albumen putrefy when coagulated. This property of creosote renders it applicable to a variety of useful purposes, and there is no better way of preserving anatomical preparations than by immersing them in a solution of creosote. M. Martin Solon has stated this in a memoir upon creosote, which he read last October, before the Academy of Medicine at Paris.

I have tried a number of experiments with it upon fresh meat, from which I have reason to believe that it may be very usefully employed in domestic economy. The meats which are intended to be prepared, ought to be immersed in a solution of one part of creosote in a hundred of water. In this solution they should remain from twelve to forty-eight hours, according to their size, when they are to be dried either in the sun or before a fire, and afterwards set aside for six or eight days, at the end of which

period they will be found to have acquired the appearance, consistence, smell, and taste of the finest smoked meat. Bullocks' tongues, mutton hams, haddocks, salmon, &c. &c. do well to be treated in this way. The tongues which I prepared in this manner equaled in delicacy of flavour the finest smoked reindeer tongues so much prized by many epicures, and the haddocks were not inferior to those preserved with wood smoke at the celebrated Finnan. Before leaving the subject of the domestic uses of creosote, it may be stated, that four drops of creosote added to a gallon of whiskey, impart to it a peculiar flavour, very similar to that well-known by the name of the *peat-reek*. Dealers have long been in the habit of giving the peat-reek taste to whiskey, with a view of making their article pass in the market as the much-prized product of the small Highland stills, by adding to it a minute quantity of the oil procured by the distillation of peat in close vessels. Since creosote has been introduced into this country, it has been substituted for the peat-oil, and is at present extensively used for this purpose, both by dealers and in some private families, as I have occasion to know. The knowledge of these circumstances led me some time ago to undertake some experiments, with a view of discovering the best method of obtaining creosote from peat-tar, and I regret that I am as yet unable to give the result; but it may be safely stated, that it is peculiarly rich in this substance.

Birds poisoned with creosote resist putrefaction for a great length of time, and the bodies of animals may be mummified so as to keep in a sound state for an indefinite period, by immersing them in a solution of creosote in water, or by injecting a mixture containing creosote into the blood-vesssels, and the embalmment may be rendered more complete, by filling the cavities of the cranium, thorax, and abdomen with the same solution. There can be little, if any doubt, that bodies preserved in this way would resist the destroying effects of time as effectually as the most ancient of the Egyptian mummies have done. I have in my possession the lungs of a dog which I poisoned with creosote three months ago, and they are as fresh at this moment as when cut out of the body, and still exhale a strong odour of the substance. Since the discovery of creosote, it has been justly considered the antiseptic principle of wood smoke, impure pyroligneous acid, and tar-water; and any one who pays a little attention to the vast researches which have been made into the subject of Egyptian embalming, must be persuaded, that to it we are indebted for those strange relics of antiquity called mummies. As this is a subject of much interest, a separate chapter may with propriety be devoted to its investigation.

CHAPTER III.

ON THE EGYPTIAN EMBALMING.

The accounts given us of the process of embalming, by Herodotus and Diodorus Siculus, are the most copious which we possess, and on a cursory perusal may seem clear and circumstantial, but they are in reality extremely defective and unsatisfactory. This is not to be wondered at, for we can easily imagine, that it was the most anxious endeavour of the embalmers to veil their sacred art in mystery; and, in all probability, the various spices which they used were employed chiefly for the purpose of concealing the odour of the mummifying drug.

Herodotus gives us the following account of the process: "In the most perfect specimens of their art, they draw the brain through the nostrils, partly with a piece of crooked iron, and partly by the infusion of drugs. They then, with an Ethiopic stone, make an incision in the side, through which they extract the intestines; these they cleanse thoroughly, washing them with palm wine, and afterwards covering them with pounded aromatics. They then fill the body with powder of fine myrrh, cassia, and other perfumes, except frankincense.[1] Having sewed up the body, it is covered with nitre for the space of seventy days, which time they may not exceed. At the end of this period it is washed, closely wrapped in bandages of cotton, dipped in a gum which the Egyptians use as a glue. It is then returned to the relations, who enclose the body in a case of wood, made to resemble a human figure, and place it against the wall, in the repository of their dead. The above is the most costly method of embalming. They who wish to be less expensive adopt the following method: They neither draw out the intestines, nor make any incision in the dead body, but inject an unguent, made from the cedar. After taking proper means to secure the injected oil within the body, it is covered with nitre for the time above specified. On the last day they withdraw the liquor before introduced, which brings with it all the bowels and intestines. The nitre eats away the flesh, and the bones and skin only remain. The body is returned in this state, and no further care taken concerning it. There is a third mode of embalming appropriated to the poor. A particular kind of ablution is made to pass through the body, which is afterwards left in nitre for the above seventy days, and then returned."[2]

Before offering any remarks upon this passage, it is better to add the account furnished by Diodorus Siculus: "They who have the charge of wrapping up and burying the body, are such as have been taught the art by their ancestors. These give in a writing to the family of every thing that is to be laid out in the funeral, and enquire of them after what manner they would have the body

[1] Frankincense was not used, because it was consecrated to the gods.
[2] Herodot. Euterpe.—Beloe's Translation.

interred. When every thing is agreed on, they take up the body, and deliver it to them whose office is to take care of it. Then the chief among them, who is called the scribe, having the body laid upon the ground, marks out how much of the left side, towards the bowels, is to be incised and opened, upon which the *paraschistes*, (so by them called,) with an Ethiopian stone, dissects so much of the flesh as by the law is justifiable, and having done it, he forthwith runs away, might and main, and all those present pursue him with execrations, and pelt him with stones, as if he were guilty of some horrid offence, for they look upon him as an hateful person, who wounds and offers violence to the body in that kind of way, or does it any prejudice whatever. But as for those whom they call the *tarichcutæ*, (the embalmers,) they highly honour them, for they are the priests' companions, and, as sacred persons, are admitted into the temple. As soon as they come to the dissected body, one of the tarichcutæ thrusts up his hand through the wound in the breast of the dead, and draws out all the intestines but the veins and the heart. Another cleanses all the bowels, and washes them in Phœnician wine, mixed with divers aromatic spices. Having at last washed the body, they first anoint it all over with the oil of cedar, and other precious ointments, for the space of thirty days together; that done, they rub it well with myrrh, cinnamon, and such like things, not only apt and effectual for long preservation, but for sweet scenting of the body also, and so deliver it to the kindred of the dead, with every member so whole and entire that no part of the body seems to be altered till it comes to the very hairs of the eyelids and eyebrows, insomuch as the beauty and shape of the face seems just as it was before. By which means many of the Egyptians, laying up the bodies of their ancestors in stately monuments, perfectly see the true visage and countenance of those that were buried many ages before they themselves were born."[1]

There is so much manifest absurdity in the account of the method of embalming given by both of these authors, that we must look for light upon the subject, rather to the writings of those numerous antiquaries and travellers, who have in modern times with so much care and industry examined mummies, and searched into the nature of the medicaments which have been found in them.

That the brain could have been extracted through the nostrils in the manner described, is barely possible; and the assertion, that clysters of cedria, or tar oil distilled from the cedar, brought away with them the bowels, is utterly preposterous; and the statement that mummies were placed in an erect position, is now known to be erroneous.[2] It would be easy to bring forward many proofs of the numerous inaccuracies of these credulous historians; but this is unnecessary, as it is sufficient to state, that all the attempts at embalming according to the methods given by them, have proved completely abortive, showing very clearly that they have omitted to mention some essential part of the process.

[1] Book I. chapter vii.—Booth's Translation.
[2] See Travels of Belzoni and others.

That historians should have been unable to get at the true art of the embalmers, is just what we would naturally expect from the peculiar care which the embalmers took to conceal their secrets from the world. Their chemical books were all written in secret characters, and deposited in the innermost recesses of their temples.[1] That chemistry was highly cultivated by the Egyptians, is evident from the allusions to the subject by various authors both ancient and modern, and Borrichius, among others, has written some learned, and now very scarce volumes, expressly on the subject.

Pliny speaks of their beautiful pigments;[2] and from what we in the present day have had an opportunity of observing, we know that their colours are of the most extraordinary durability, as is exemplified by finding the nails of mummies generally dyed of a reddish hue. Baron Denon, describing an ornamented mummy case, says, "De laps d'un moins quarante siècles, les couleurs en sont encore très vives; il y a du vert, du jaune, du rouge, et de l'orange."[3] And Pettigrew, speaking of mummy cases, says, "The colours with which they are decorated have retained their liveliness and beauty in a most surprising manner. The green is the only colour which appears to have faded; it is sometimes compounded with the blue, though blue is metallic, the yellow vegetable. The nature of the white, which is most durable, has not been discovered. The red is very brilliant. Red, blue, yellow, green, white, and black, are the colours to be found both on the cases and on the walls of the tombs; and it is stated in a note, that M. Champollion has observed a violet colour painted in the bas reliefs."[4] It is not a little singular, that in the coffin of a mummy described by M. Passalacqua, there was found a small vase containing a mineral matter with which at the present day the Coptic ladies paint their eyebrows.[5] Seneca and other authors inform us that they made artificial gems of great beauty and value.[6]

But what is more to our present purpose is, that they were well acquainted with those operations in which fire performs the most important part. Zosmius, an ancient chemist of Panopolis, has given descriptions and representations of their retorts, and other vessels for distillation; and that they understood the smelting and working of gold and silver, is matter of common notoriety. It is sufficient to allude to the ornaments formed of these metals found in their tombs, and to their gold and silver statues, of which historians inform us; and we may also refer to the gold chain which Pharaoh placed around Joseph's neck. The knowledge of the

[1] Volney informs us, that about the year 1780, upwards of one hundred volumes, written in an unknown language, were dug up near Damietta, which were immediately committed to the flames by command of the sheiks of Cairo. Vol. i. p. 285.

[2] Pliny, Lib. xxvii. [3] Denon, T. i. p. 700.

[4] Pettigrew's History of Egyptian Mummies. Quarto, London, 1834, page 116.

[5] Ibid. page 107. [6] Seneca, Epistle xc.

science of chemistry, as well as that of many useful arts, was completely eradicated from among the Egyptians by the terrible burning of temples and libraries, which attended the invasion of their Persian, Ethiopian, and Roman foes.

It appears that all the substances found within mummies are of a resinous nature. That the mere introduction of these substances into the great cavities of the body, along with external lotions of Phœnician wine, &c., would answer the purpose of embalming, we know to be contrary to fact. Taking, however, the accounts given us by the historians already quoted, in connection with the examination of mummies by modern observers, we are enabled to form what seems a pretty correct opinion as to what was the real secret of the embalmers; and it is hoped that the following observations will make it apparent that creosote was the mummifying drug.

The grand omission in the description of the process given us by Herodotus and Siculus, is their making no allusion to the application of heat; but that this formed an essential and constant part of the operation, we have good reason to believe; and of the certainty of this fact, M. Royer was perfectly convinced; for he says, when speaking of the application of heat, "Cette operation, dont aucun histoire n'a parlé, etoit sans doute la principale et la plus importante de l'embaumment."[1] It would be easy to bring forward a mass of evidence in support of this opinion were it necessary; but it is sufficient to refer generally to all the published accounts of the unrolling of mummies. With a view, however, of showing that the degree of heat employed in the mummifying process was extremely great, we subjoin the following notices.

In an account of a mummy inspected at London in 1783, which was published in the Philosophical Transactions of the following year, we are told that the ribs were so *black and burned*, that they could with difficulty be distinguished from the pitchy matter in which they were imbedded, and that the bones of the spine and pelvis were in the same state, only more burned. In a mummy which was brought from Egypt, by Baron Denon, it was observed that the humerus seemed to have been forced aside, and the cavity of the shoulder filled with bitumen, which bore evident marks of having been poured in while in a heated state.[2] Pococke, describing a mummy, says, "There were four folds of cloth over the head, the upper one being painted blue; under this there was a composition about half an inch thick, as I imagined, of gum and cloth, that was *burned* by the heat of the things applied to it;"[3] and in another part of the same description he says, "that the bituminous matter had penetrated into the diploe of the skull, but that little or no trace of it could be seen on the outer table of the bone." This was in all probability the effect of great heat upon the resinous matter within the body. Such a phenomenon might certainly

[1] Descript. de l'Egypte, p. 212.
[2] London Lit. Gazette, No. 705, July 24, 1830.
[3] Pococke's Travels, vol. i. p. 230.

be accounted for by supposing the blood-vessels to have been injected; but there does not seem to be sufficient ground for supposing that this was done. The whole muscular tissue of mummies is generally found impregnated with the embalming material. Belzoni, speaking of this impregnation, says, "What does not incorporate with the fleshy part, remains of the natural colour of the pitch, but that which does incorporate, becomes brown, and evidently mixed with the grease of the body."[1]

These facts, to which many more of the same nature might be added, were it necessary, establish most clearly that there was a very strong heat applied to the body during the process of embalming.[2]

As supplementary to what has been stated, and as tending most powerfully to corroborate the views already expressed, we must advert to the account given of the embalming of Asa, in 2 Chron. xvi. 14. "And they buried him in his own sepulchre which he had made for himself, in the city of David, and laid him in the bed which was filled with sweet odours and divers kinds of spices, prepared by the apothecaries' art; and they made *a very great burning* for him." This "*very great burning*" was clearly connected with the embalmment, and as the Jews acquired their knowledge of the method of preserving dead bodies during their residence in Egypt, there can be but little doubt but that the "*great burning*" formed also a part of the process as practised there.

Since it appears from the accounts of the ancients, and the investigations of modern mummologists, that the embalmers introduced large quantities of bituminous matters into the great cavities of the body, and as we have shown that the application of heat formed part of the process of embalming, it must of necessity have followed that creosote was generated, and propelled through all the tissues of the body. It may be stated, then, as a summing up of what has been here said on the subject, that the application of such heat as would first dry up the body, and then decompose the tarry matters which had been previously introduced, and thus generate creosote, formed *the only essential part of the mummifying process;* that the spices and perfumes used were superfluous; and that the various other operations connected with the embalmments, of which we are told, were mere matters of idle ceremony, and were had recourse to, in all probability, chiefly with a view of mystifying the notions which the vulgar might entertain of the embalmers' secret art. That the Egyptian embalmers were aware of the cause of the importance of the application of heat, is highly probable, from the advanced state in which chemistry was among them, and to which reference has already been made; but be this as it may, it does not render it less clear that, during the mummifying process, creosote

[1] Belzoni's Travels, vol. i. p. 260.

[2] Too intense a heat seems occasionally to have been employed. Pettigrew tells us that a female mummy which he unrolled, was "destroyed by the excessive heat with which the applications had been made, and that the bandages were literally *burnt to tinder*."

was generated. It would be out of place to enter into a lengthened discussion regarding the substances used in embalming, as it is sufficient for our purpose to state that the resinous and bituminous matters infinitely predominated over all the rest. It appears that the native mummia or mineral tar was used as well as the vegetable. The cedria, or cedrium of Pliny, which he says was used in Egypt for embalming, unquestionably contained ready-formed creosote, as all oils of tar do; for the process by which tar was obtained is almost identical with our own. "Pix liquida Europa e taeda coquitur, navalibus muniendis multosque alios ad usus. Lignum ejus concisum furnis undique igni extra circumdato fervet primus sudor, aquae modo fluit canali, hoc in Syria cedrium vocatur; cui tanta vis est, in Egypto ut corpora hominum defunctorum eo perfusa serventur."[1] The liquid called cedria seems to have been prepared from other trees as well as the great cedar, and appears to be a term sometimes merely denoting that the substance spoken of was possessed of preserving qualities; for, from the great durability of cedar wood, it was the hieroglyphic of eternity.[2]

CHAPTER IV.

CREOSOTE THE ANTISEPTIC PRINCIPLE OF PEAT.

Some consider the carbonic, gallic, and other acids, as the principal active antiputrescent agents which reside in peat, while others regard as such the saline substances which it contains; and others, certainly with more probability, attribute its embalming power to the bituminous matters which are found in it. It is a very common occurrence to see a thin film of oil floating upon the surface of peat water; and in some mosses this is very abundant, and every kind of bitumen has been found in peat, from the coarsest mineral tar, to the purest naphtha. Where these substances most prevail, the antiseptic power of the peat is strongest. That this depends upon the presence of creosote, there is every reason to believe. To enter into any discussion here, as to the manner in which the vegetable matter is decomposed when it becomes peat, would be out of place, especially as it could not be done without entering into a very wide field of controversy. It is enough, however, to state, that as these bituminous substances which are contained in it are antiseptic, and as bituminous matters in general contain creosote, there is no difficulty in understanding why it should be creosote which renders peat antiseptic. That creosote

[1] Pliny, Lib. xvi. cap. 21. In usum Delph. 1826.

[2] From this circumstance, and from its own intrinsic virtues, the leaves and binding of books were smeared with it, so that it became a proverbial expression when speaking of one whose fame deserved to descend to posterity—"*Cedro digna locutus.*"

can be derived from peat by destructive distillation, is also a collateral proof of the truth of this proposition, and that heat assists in the formation of peat, is not altogether a fanciful doctrine, although there are opposed to it many great names. The heat produced by the spontaneous chemical changes going on in the vegetable matter, is sometimes very great, and in consequence of them great peat mosses have been known to take fire. It is probable, that the more heat that has been in force during the formation of peat, the more bituminous it is, and the higher, consequently, are its antiseptic properties. An interesting fact, confirming this opinion, is, that in warm climates we find no peat, but in those situations where it might be expected we have in its place a highly bituminated substance—the asphaltum; and in those regions, also, the liquid mineral matters, known by the several names of oil of petroleum, naphtha, native mummy, &c. are far more abundant than in cold and temperate latitudes. There are no reptiles found in quick mosses, and they are also destitute of vegetation, and fishes and insects are never found in peat water. Like creosote, however, peat water may be advantageously used in medicine; and, in some cases, living in an atmosphere impregnated with peat smoke is of great use in the counteraction or alleviation of disease; and there is some reason to believe that all these effects are produced by the presence of creosote.

CHAPTER V.

PREPARATION OF CREOSOTE—ITS ADULTERATIONS.

Since Reichenbach published the process which he followed in preparing creosote, various improvements have been suggested by other chemists. He states, that his own attempts to abridge and simplify the tedious and troublesome process, which he has published in the German journals, were quite unsuccessful, and recommends every one to adhere to the old method, as the substance procured by an abbreviated process could not with safety be given internally. He adds, that at one time he thought he had succeeded in procuring creosote by a simplified process, though not chemically pure, yet sufficiently so for medicinal purposes. Under this impression, he delivered some of it so prepared to several practitioners; but he soon had reason to regret this, for he was informed that, in the cases in which it had been administered, the most frightful vomiting had ensued. This remarkable effect he attributes to the presence of a peculiar emetic principle, of such activity that when half a drop of it is put upon the tongue vomiting is produced. The symptoms observed in the cases where it had been adminis-

tered were great nausea, flushing of the face, a fixed and projecting appearance of the eyes, and then followed repeated attacks of vomiting, which were succeeded during the remainder of the day by great prostration of strength. It is strange, and much to be regretted, that Reichenbach, in his anxiety to warn the druggists against making use of an abbreviated process in the manufacture of creosote, should have entirely omitted saying a single word about the chemical and physical properties of this, till then, unheard of substance.[1]

But, although Reichenbach has failed in making any improvements upon the process which he first published, other experimenters have not been so unsuccessful, and various chemists have devised processes of a more simple nature than his.

As far as I am able to judge, the best yet published is that in the Annales de Chimie et Physique of July, 1835, by M. Koene. This gentleman attempted to get creosote from tar, according to the process of Reichenbach, but obtained so small a quantity that he did not think it worth his while to purify it.

The process which he gives is as follows:—The tar derived from pit-coal is distilled in a retort provided with a long tube having a large mouth. Under this is placed a receiver. The oil which comes over first swims on water; and it is necessary to remove from time to time the products of the distillation, till an oil is obtained which sinks in water. When this is found to be the case, the product is collected. The heavy oil obtained during the distillation condenses not only in the receiver, but also in the tube of the retort, where it unites with the naphthaline, forming a buttery substance. By applying a gentle heat, the mass will drop into the receiver. The product is now allowed to remain in a cool place for some hours, after which it is pressed. The expressed naphthaline still contains oil, which is separated by heating it with its own weight of acetic acid till it melts. After allowing it to cool, the crystalised naphtha is pressed, and the acid adhering to the creosote is saturated with subcarbonate of potash. The creosote is now to be shaken for a quarter of an hour with phosphoric acid, the proportions being half an ounce of the acid to twenty ounces of the oil. The mixture ought then to be agitated with its volume of water, and afterwards distilled with a graduated heat—care being taken to separate the oil which floats on the surface. The rectified oil is now to be dissolved in its own volume of a hot solution of caustic potash, of specific gravity 1.120. When it has been allowed to cool for half an hour, the supernatant oil is again removed, and the heavy oil again treated with the caustic potash—only a fourth part, however, of the solution being this time employed.

On uniting the solutions of potash, a slight excess of diluted phosphoric acid is added, and the free creosote which floats on the surface is separated. It is again rectified; and the first product,

[1] Bulletin Gén. de Thérapeutique, Dec. 1833.

which is chiefly water, being rejected, the creosote comes over quite pure.

M. Koene recommends the substance thus prepared to be preserved in bottles, covered with black paper.

The protracted process required for obtaining creosote (and it must necessarily be a long and complicated one) will always make it an expensive drug, and consequently liable to adulteration or careless preparation. The quantity of creosote obtained by the above process by M. Koene, from a "*litre*" (32 ounces) of tar, was ten "*gros*" (drams). M. Lémére, one of the first Parisian druggists who made pure creosote, obtained from eight hundred pounds of tar about six pounds of creosote.

M. Koene states, that he examined a substance which it was pretended was creosote, and found it to consist of the heavy oil of tar several times rectified. It merely contained a trace of creosote: it was obtained from the tar of pine. Having separated the creosote by means of the solution of potash, and removed the eupione by repeated distillations, he found the substance to be destitute of smell, of the consistence of almond oil, and with a specific gravity of 1.009. It became brown on exposure to light, was not so soluble in alcohol as creosote, and its alcoholic solution became less milky on the addition of water. Hydrochloric acid did not alter it. Nitric acid gave it a less intense yellow colour. Concentrated sulphuric acid, in the cold, gave it a brownish black colour, without total decomposition; but when this mixture was boiled, decomposition took place. As this oil dissolves very easily in creosote, it renders this species of adulteration very easy. The fraud may be easily detected by treating the suspected liquid with acetic acid, or a solution of caustic potash; and in either case the creosote will be dissolved, when the oil will remain unaltered.

Very analogous to this adulteration, is that with capnomor. This is not a fraudulent deterioration, any farther than that it is the duty of those who make creosote for sale to ascertain by experiment, before bringing their article into the market, that it is free from this and every other impurity. From the similarity of this substance to creosote, both in chemical and physical properties, (as may be seen by referring to pages 193, 194, of this treatise,) it is easy to account for the frequency with which it is found associated with the creosote of the shops. This is a circumstance worthy of the attention both of practitioners and druggists; as by a knowledge of the manner of putting the matter to the test, the latter may be prevented from paying the price of pure creosote for what is in reality a spurious article, and the former from being perplexed with the inefficacy of a drug which he believes to be identical with that which in similar circumstances he had employed with marked advantage.

The presence of capnomor in creosote may be recognised, *first*, by the less specific gravity of the suspected specimen; *second*, by adding to it acetic acid, when the creosote will be wholly dissolved, but the capnomor will be hardly at all acted upon; and, *third*, by

observing what takes place when it is added to a strong alkaline solution, in which creosote is soluble, but capnomor wholly insoluble.

The creosote made by Reichenbach is derived from beechwood tar, but a considerable proportion of that manufactured in this country is obtained, I believe, from the tar of the coal-gas works. It can easily be distinguished from that got from beech tar, by a peculiar, somewhat fetid, and ammoniacal odour. The creosote procured by Koene from pit-coal tar had more the smell of castor, he informs us, than of smoked meat, but was in all its chemical properties identical with creosote.

From creosote uniting with almond oil, it is probably sometimes adulterated with this oil, as the greater density of the fluid might easily pass unnoticed by the inexperienced. There is a peculiar principle which obstinately adheres to creosote, and which, from its combining with great avidity with the oxygen of the atmosphere to form a brown-coloured substance, gives the substance a brown tinge; but the existence of this shade of colour is no reason to avoid giving it internally, as its presence does not seem to influence its therapeutic agency in any degree. At this moment there is probably none in any of the shops of this city which has not a decided brown tinge; and the only specimen destitute of it which I have seen, is one which I obtained from Dr. E. D. Allison, the druggist, last summer, which he received from the continent, and which he believed to have been manufactured by Reichenbach himself.

Creosote was procured by Reichenbach in the following manner, from crude pyroligneous acid, at a temperature of 70° or 80° F. The acid is neutralised by the addition of sulphate of soda. This operation causes a quantity of oil to be separated, which, on standing for some days, is seen floating on the surface; and at this time a fresh portion of pyroligneous acid, and sulphate of soda, is again separated. It is then saturated with carbonate of potash. A thick oil is separated, which is to be distilled with water; when this is done, a pale yellow-coloured oil is obtained, which is to be frequently agitated with fresh portions of diluted phosphoric acid. The liquor is then left at rest for some time, after which it is treated with a fresh quantity of water, till there ceases to be any acid reaction. The liquid is now distilled with a fresh quantity of water charged with phosphoric acid, care being taken to cohobate from time to time. The colourless oil contained in the receiver is now to be dissolved in a solution of caustic potash, of a density of about 1.12, when the eupione which is on the surface is drawn off, and the heavier oil, which is chiefly creosote, is put into a shallow vessel, and left exposed to the action of the air until it becomes brown; or this part of the process may be hastened by gentle ebullition in an open vessel. This change of colour is owing to the oxidation of the peculiar substance already mentioned which it contains. The liquid is now saturated with sulphuric acid, and again distilled till a bituminous residue is left in the retort. The solution

in the caustic alkali, and the subsequent manipulations, must be repeated until the oil ceases to become brown on exposure to the air. In conducting all the distillations, it is necessary to guard against any drops condensing on the sides of the retort, as they might be decomposed by the action of the fire. By this method creosote may be obtained in great purity, but not with sufficient ease, nor in sufficient quantity, to recommend the process to the manufacturing chemist.

PART SECOND.

HISTORY OF THOSE SUBSTANCES WHOSE MEDICINAL PROPERTIES SEEM TO DEPEND ON THE PRESENCE OF CREOSOTE—THE PHYSIOLOGICAL AND MEDICINAL PROPERTIES OF CREOSOTE.

CHAPTER I.

MUMMY, HUMAN AND MINERAL—THE MANTEY OF THE ARABS, CEDRIA, TAR, SOOT, EAU DE BINELLI, &C. &C.

From the great repute in which the mummified flesh of dead bodies has been held at various periods, and in almost every country, as a remedy for the very same diseases in which creosote has been recommended in modern practice, it seems proper to give a few historical details regarding this very curious subject; the more especially as this remarkable coincidence may be explained by the hypothesis already stated—viz., that in the embalming process creosote was generated, and propelled into every tissue.

Mummy seems to have been officinal under a variety of forms. In the Pharmacopœia Schrodero-Hoffmaniana there are given formulæ for preparing tinctures and oils from this substance.[1] In the Pharmacopœia Augustana Reformata it forms part of a variety of powders, plasters, and cerates.[2] Olaus Wormius tells us that it was frequently taken in the form of powder, and that in this state it was administered in doses of half a dram.[3] We are informed, by Belon, that Francis the First was in the habit of carrying about his person a powder composed of mummy and rhubarb, to be used in case of his meeting with any accident from falls or otherwise.[4]

[1] Geneva. Folio, 1687. p. 609.
[2] Dordrechti, 4to. 1772. pp. 163, 384, 392, 397, 402, 403, &c.
[3] Museum Wormianum. Folio, Amsterdam, 1655. p. 344.
[4] Observations de Plusieurs Singularités, &c. p. 261.

Mummy has been used in medicine from the earliest ages. It is probable that its virtues were discovered in the reign of King Feridu'n, who flourished, according to Sir William Jones, in the eighth century before Christ; or, according to others, about two thousand years before the Christian era.[1] It was very extensively prescribed by Emalgar, a famous Jewish physician, who flourished about 1100 A. D., (or, according to others, about 1300,) both for the Jews and Christians then in the East contending for the Holy Land.[2] From that time it became a favourite remedy all over the world with the professors of the healing art.

Avicenna recommends it in cutaneous diseases, fractures, abscesses, contusions, nausea, dyspepsia, hemoptoe, ulcerated lungs, palpitation of the heart, and a variety of other maladies.[3] Olaus Wormius praises it as a remedy in spasmodic affections, contusions, wounds, and in external and internal ulcerations. He and others speak highly of its efficacy in gangrene.[4] Lemery says, that it is "detersive, vulnerary, resolvent, a remedy against gangrene, strengthening, well adapted for contusions, and for preventing the blood from coagulating in the body."[5] And many other physicians of ancient note speak with great confidence of the therapeutic virtues of this drug.

No where was it so much in vogue as in France, but in our own country it seems to have attained considerable celebrity. It has a place in all our old books on the materia medica. Hill gives us the following account of it.

"We have two different substances preserved for medicinal use, under the name of *mummy*, though both are in some degree of the same origin. The one is the dry and preserved flesh of human bodies, embalmed with myrrh and spices; the other is the liquor running from such mummies, when newly prepared, or when effected by great heat or by damps. This latter is sometimes in a liquid, sometimes in a solid form."[6] Dr. James says—"Under the name of *mummy* are comprehended, first, the mummy of the Arabians, which is a liquament or concreted liquor, obtained in sepulchres by exudation from carcasses embalmed with aloes, myrrh, and balsam. If this mummy could be procured right and genuine, it would be preferable to the other sorts. The second kind of mummy is the Egyptian, which is a liquament of carcasses seasoned with pissasphalt."[7]

[1] Dict. Buchán Kútea, as quoted by Ouseley, vol. ii. p. 481.
[2] Pettigrew, p. 7.
[3] De Viribus Cordis. Folio, Venete, 1608, tom. ii. 384.
[4] Mus. Wormianum, p. 344.
[5] Dictionnaire des Drogues Simples. Rotterdam, 1727. p. 362.
[6] Hill's Materia Medica. London, 1751. 4to. p. 875.
[7] Pharmacopœia Universalis. London, 8vo. 1747, p. 512. This passage is cited merely as giving the commercial history of the drug, because its admission into James's work could not be considered as any proof of its having been much in repute when he wrote, as he has retained a multiplicity of preposterous nostrums, which were before his day discarded from regular practice. For instance, he says, under the word *Homo*, "The officinal

Such, then, was the substance so much extolled by many of our ancestors two hundred years ago. Lord Bacon celebrates its power in stopping hemorrhage; he says, that "it hath great force in stanching of blood, which, as it may be ascribed to the mixture of balmes that are glutinous, so it may also partake of a secret property, in that the blood draweth man's flesh."[1] Boyle also praises it as a drug.[2] We find various allusions to the use of mummies in medicine, by several of our old dramatic and other authors.

"The worms were hallowed that did breed the silk,
And it was dyed in *mummy*, which the skilful
Conserved of maiden's hearts."[3]—*Othello, Act III, Sc.* 4.[4]

Falstaff says—

"I had been drowned, but that the shore was shelvy and shallow, a death I abhor, for the water swells a man, and what a thing I should have been when I had been swelled—I should have been a mountain of *mummy*."
Merry Wives of Windsor, Act III, Sc. 5.

"But I'll have one (a tomb)
In which I'll lie embalmed, with mirrh and cassia,
And richer unguents than the Egyptian kings,
And all this that my precious tomb may furnish
The land with *mummy*."
Muses' Looking-Glass, Act III, Sc. 1.[5]

"That I might tear their flesh in mammocks, raise
My losses, from their carcasses turned *mummy*."—*Honest Lawyer*.[6]

"Make *mummy* of my flesh, and sell me to the apothecaries."
Shirley's Bird in the Cage.[7]

"The Egyptian mummies, which Cambyses or time hath spared, avarice now consumeth. *Mummy* is become merchandise, Mizraim cures wounds, and Pharaoh is sold for balsams."—*Sir Thomas Browne's Hydriotaphia.*

Though mummy is now hardly, if at all, known in European practice, it is at present, as Madden informs us, a favourite remedy with the Arabs. They mix it in powder with butter so as to form an ointment, which they call *mantey*. This they esteem a sovereign remedy for ulcers, both internal and external.[8]

simples furnished from the parts of the human body, whilst alive, are the hair, nails, saliva, ear-wax, sweat, milk, menses, secundines, urine, dung, semen," &c.

[1] Sylva Sylvarum. Cant. x. 8, 980.

[2] Boyle's Works, vol. ii. p. 451.

[3] Roquefort tells us that the bodies of young girls were considered most efficacious, and produced the greatest price. Pettigrew, p. 12.

[4] Entered at Stationers' Hall, Oct. 6th, 1621.

[5] By Thos. Randolph. He died in 1634, at the age of twenty-nine, but the precise date of the play cannot be ascertained.

[6] Pettigrew, in quoting this passage, erroneously ascribes the play to Shirley. It was published anonymously, but with the letters S. S. on the title page, in 1616. At this time Shirley was only eight years old, being born in 1608. The author is unknown.

[7] This play was first published about 1632.

[8] Madden's Travels in Turkey, Egypt, &c., vol. ii. p. 90.

Mummy did not fall into disrepute from any want of faith in its virtues, but from the universal disgust created, when it was discovered that the European market was supplied entirely by a company of Jews, who prepared a species of mummy in Egypt with the common bitumen of the country, and that for this purpose they made use of the bodies of criminals, of those who had died of the plague, or, in fact, of any upon which they could lay their hands.[1]

This was first announced to the world by Ambrose Paré, a French surgeon,[2] who made the statement upon the authority of Guy de la Fontaine, physician to the king of Navarre, who visited Egypt in 1564. He requested the principal Jew engaged in the traffic to show him his mummy depository, which was readily complied with. When making some enquiries concerning the sepulture of the ancients, the Jew unhesitatingly informed him that those mummies which he saw had all been manufactured by himself during the four preceding years, and were the bodies of slaves who might, for aught he knew, have died of leprosy or any other malignant disease; and he expressed astonishment, how "dainty-mouthed Christians" could eat dead bodies.[3]

In reference to this subject, Chambers says—"All that is sold in the shops, whether brought from Venice or Lyons, or even directly from the Levant by Alexandria, is fictitious, the work of certain Jews, who counterfeit it by drying carcasses in ovens, after having prepared them with powder of myrrh, caballine aloes, Jewish pitch, and other coarse and unwholesome drugs."[4]

From this time, the reputation of mummy seems to have been decidedly on the decline, and we find instructions given by various authors as to the manner in which the genuine mummy was to be discriminated. Lemery says, "Il faut choisir la mumie nette, belle, noire, luisante, d'une odeur assez forte, et qui n'est point desagréable; on en tire par la distillation chymique beaucoup d'huille, et de sel volatil." But when he gives these instructions, he states his conviction that what was imported into Europe was not the real mummy of the tombs. "Il ne faut pas croire que la mumie commune qu'on nous apporte, soit de *la veritable mumie d'Egypte*, qui ait eté tirée des sepulcres des anciens Egyptiens; celle-là est trop rare, et si l'on en a quelque partie, ou la garde dans des cabinets comme une grande curiosité."[5] It is expressly stated by various authors, that the medicinal powers of this substance were wholly to be attributed to the bituminous matters used in the embalmment. "Mummy," says Hill, "has been esteemed resolvent and balsamic; but whatever virtues have been attributed to it, seem to be such as would depend more upon the ingredients used in

[1] Pettigrew, p. 11. Hill's Materia Medica, p. 875. Lemery's Dict. de Drogues Simples, p. 363.

[2] Ambrose Paré has written at great length against the use of mummy in medicine.

[3] Pettigrew, p. 8.

[4] Chambers' Encyclopædia. Article *Mummy*.

[5] Lemery, Dict. de Drogues Simples, p. 363.

preparing the flesh, than in the flesh itself; and it would surely be better to give those ingredients without so shocking an addition."[1] And in the article Mummy, in Nare's Glossary, we find it mentioned, that the Dean of Westminster, in his History of the Commerce, &c., of the Ancients, says, that "it was medicinal, not on account of the cadaverous, but of the aromatic substance."

Dioscorides applies the term *mùmie* (or, as some write it, *momia*), to a species of native oil of tar or petroleum, which he describes in the following manner, as translated by Serapion. "Mumia est in terris Apolloniae; descendit namque ex montibus, qui ducunt flumina cum aqua, et ejicit eam aqua fluminis in ripis, et est coagulata, et fit sicut cera, et habet odorem picis mixtae cum asphalto cum aliquo fœtore: et virtus ejus est sicut picis et asphalta mixtorum."[2] Many authors assert that the Egyptians embalmed their dead with the natural *momia*;[3] and it seems to be the general opinion that the Persian word *nùmiâ*, signifies mineral pitch, although some consider its primary meaning to be an embalmed or aromatised body.[4] There seems, however, to be sufficient reason for believing that both the mineral and vegetable tars were employed by the embalmers.

The celebrity of the native mummy, in medicine, was very great, and it was applied to the very same purposes as the mummy of the tombs, and as creosote. Pliny says that a mixture of this substance was useful in itch and mammary excoriations. "Est et pissasphaltos mixta bituminis pice naturaliter in Apollionatarum agro. Quidem ipsi miscent praecicipuum ad scabiem pecorum remedium aut si fœtus mammas laeserit."[5] There was believed to be great diversity in the medicinal powers of the native mummy, dependent upon the place where it was obtained. Alluding to this, Pliny says, "Theopompus scripsit in Appolionatarum agro picem fossilem non deteriorem Macedonia inveniri."[6] In Sylvestre de Lacy's translation of the works of Abd'-Allatif, an Arabian physician of the twelfth century, we find the following notice of this remarkable substance. "La momie sort de source comme la poix minerale, la napthe, d'autres disent que la momie est un varieté de poix minerale, et on la nomme *menstrues des Montagnes*."[7]

Chardin tells us that though the Persians are acquainted with the mummy of the tombs, they set by far the highest value upon the native mummy, which they term moùmènêhy or moùmiahí. Kaempfer gave the preference to the native substance as a medicine, and believes that it was to be found in the bodies of those Egyptian mummies of antiquity who had been princes and illustrious men.[8]

[1] Hill's Materia Medica, p. 876.
[2] De Simplici Medicina. Venice, 1503, p. 138.
[3] All Egyptian mummies were certainly not embalmed with the native bitumen, but the laborious investigations of Rouelle, and others, seem to prove that it was extensively used. This is also the opinion of Kaempfer. *Vide* his Amœnitat. Exot. p. 520.
[4] Pettigrew, p. 1. [5] Pliny, lib. xxiv. cap. 25. [6] Ib. lib. xvi. cap. 23.
[7] Rélation de l'Egypte, 4to. Paris, 1810, p. 201.
[8] Kaempfer Amœnitates Exoticæ, 4to. Lemgoviæ, 1712, p. 520.

Chardin describes it as a bituminous substance which exudes in minute quantities from the rocks, resembling shoemaker's wax in appearance, colour, density, and ductility; but states, that before it has dropped from the rock it is more fluid. It is destitute of odour, and resembles, in substance, the Egyptian mummy.[1] Those authors, such as Kaempfer and others, who have written at any length on this subject, describe two species of native mummy. The one is tolerably abundant in certain mountainous districts, especially in the territory of Lar and Darab, but is considered of little value compared with the product of the *Kûh Mumiây*, or mummy mountain, at Darab, in the territory of Darabgerd, in Persia. It is found in a narrow cave at the foot of Mount Caucasus,[2] which is kept shut, and is guarded by persons appointed for that purpose by the king of Persia.

We are told by Ouseley, on the authority of the *Seir al beldam*, a book of the tenth century, that this precious substance was annually gathered for the king.[3] When the moúméněhy was collected, it was carefully sealed up in presence of the priests and magistrates, and conveyed by them to the royal repositories. This the *Seir al belad* says is the only genuine mummy; that in the hands of others being a counterfeited mixture.[4] The revolutions and wars which for so long a time convulsed Persia, seem to have withdrawn the attention of the ruling powers from the precious product of the Caucasian cave; but since the beginning of the seventeenth century it has, as formerly, been collected with much pomp and ceremony. The quantity annually procured is rather more than five *misqâls*.[5]

The Persian physicians of the present day, consider the mumia, which exudes from the *Kûh Mumiây*, as of infinitely greater value than gold. In 1809, Mirza Abul Hassan brought to our late Queen Charlotte, as a valuable present from the king of Persia, a portion of this substance.[6]

Sir William Ouseley mentions that a person at Ispahan demanded from one of his party nine *tomans*, or £8 sterling, for as much as a common sized walnut shell might have contained.[7] The empress of Russia received, as a royal present, about an ounce in a gold box;[8] and the Persian ambassador presented Louis XIV. with two golden boxes filled with this substance. Bomare gives us the following account of the presentation speech:—"L'ambassadeur de Perse dit à Louis XIV. que le baume momié etoit un specifique pour les fractures des os, et generalement pour toutes les blessures; qu'il etoit employé pour les maladies et ulcéres tant internes qu'

[1] Chardin, Voyages en Perse. Tom. iii. p. 212.

[2] A substance very similar is found issuing from the crevices of rocks in the island of Sumatra, and other places in the East Indies. The Abbe Fortis gives a very interesting and curious account of a mine of it in the island of Bua. It is there much celebrated in the treatment of paralytic affections.

[3] Volume ii. p. 113. [4] Ouseley, vol. ii. p. 114.

[5] Chardin, Voyages en Perse. Tom. iii. p. 309.

[6] Ouseley's Travels, vol. ii. pp. 121 and 478. [7] Ibid.

[8] M. de Ferrieres Sauvebœuf Mem. Hist. &c. de Voyages, tom. ii.

externes; en un mot qu'il avoit la proprieté de faire sortir le fer qui pourroit étre resté dans les blessures."[1] And Father Angelo says, "La momia che stilla dà un monte vicino à Lar e cosa preciosa; basta una mesa dramma per sanar in 24 hore un huome caduto dà alto et tutto rotto."[2]

Kaempfer speaks extravagantly in praise of it as a remedy in fractures, and relates an experiment in which he broke the leg of a chicken and applied mummy. From the result, he was satisfied of its having this property.[3]

Barbadoes tar, and all the petrolea, are applied to the same purposes as those substances already mentioned; and in various parts of Great Britain there are found mineral tars, which are used medicinally by the common people, and sometimes, also, by regular practitioners. Lewis, after speaking of the virtues of petroleum in nervous diseases, as a diuretic, an external stimulant in rheumatism, palsy, and chilblains, says, "In these intentions, some mineral oils, procurable among ourselves, are used by the common people, and often with benefit; the empyrical medicine, called British oil, is of the same nature with the petrolea, the genuine sort being extracted by distillation from a hard bitumen, or a kind of stonecoal found in Shropshire,[4] and other parts of England."[5] At St. Catherine's, near Edinburgh, there is a well which is called the *oily well*, from the circumstance of a quantity of petroleum being found on the surface of its water. This the country people use with success in itch, and other external diseases. I am not aware that they administer it internally.

In the Pharmacopée de Lyons, *naphtha* is recommended in caries of the teeth,[6] and Salmon, a noted empiric, who wrote about the end of the seventeenth century, says, "that outwardly applied, it *helps the toothache*."[7]

Preparations of *wood-tar* have been known in medicine from the earliest ages. These substances have, both in ancient and modern times, been applied to the very same purposes as creosote. Pliny mentions a variety of tarry substances as being used in medicine, such as *cedria*, *pissinum*, &c. &c. He recommends cedria, or the oil of tar, got from the cedar, in toothache. His words are—"Cedrus magna—dat picem, quae cedria vocatur, *dentium doloribus utilissimam*."[8] Galen says, when speaking of cedria, "Sicut sibi dentium foraminibus instillatur, siquidem *dolores eorum mitigat*."[9] We find this substance much talked of in books of compa-

[1] Dict. d'Hist. Naturelle, tom. viii. p. 542. Lyons, 1791. 8vo.
[2] Gazoph. Persica.
[3] Amœnitates Exoticæ. Lemgoviæ, 1712. 4to, p. 523.
[4] Philosophical Transactions of London. Vide Ap. No. 1.
[5] Lewis's Experimental History of the Materia Medica. London, 1791. Vol. ii. p. 213.
[6] Pharmacopée de Lyons. A Lyons, 1786, p. 118.
[7] Salmon's Complete English Physician, or Druggist's Shop laid open, first published in 1693. *Vide* article *Naphtha*.
[8] Pliny, lib. xxxiv. 11.
[9] Galeni de Simp. Med. Facult., lib. vii. Folio, Venetiis, 1609.

ratively recent date; and it is a curious fact, that in the Pharmacopée de Lyons, the "*oleum stillaticum ex ligno cedri*" is said to cure vomiting, one of the most valuable and striking properties of creosote. What were the reputed virtues of cedria, appear from the following passage:—"Le défaut d'observation nous empêche de convenir que la résine de cèdre calme le vomissement, par de matières pitouiteuses, qu'elle excite le cours des urines, provoque le flux menstruel, chasse le graviers contenue dans les voies urinaires, dissipe les obstructions du foie, et de la rate, qu'extérieurement elle contribue à la résolution des tumeurs insensibles, deterge et cicatrise les ulcéres."[1] Speaking of this substance, Pliny says, "Defuncta corpora incorrupta ævis servat, viventia corrumpit: mira differentia cum vitam auferat spirantibus defuntisque pro vita sit."[2] And in another place, "cui tanta vis est ut in Egypto corpora hominum defunctorum eo perfusa serventur."[3] He describes *pissinum* in the following manner:—"E pice fit quod pissinum appellant, cum coquitur velleribus supra halitum ejus expansis atque expressis; probatum maxime e Brucia, est enim pinguissima et resinosissima."[4] In another place, speaking of the medicinal properties of pissinum, he says, "Usus ad tussim, et quadrupedum scabiem est."[5]

Rayer recommends in various skin diseases, an ointment of one part of tar to four of hog's lard.[6] Bateman, when speaking of ichthyosis, says that pills made of pitch, afford one of the most effectual means of controlling the languid circulation, and the inert and arid condition of the skin;[7] and I am informed that in the hospital at Hamburgh pitch pills are at present in great repute for the cure of skin diseases. *Tar water* has been found useful in such affections, in dyspepsia and other diseases; but its fame, though at one time great, was short lived, on account of the absurdly extravagant praises of the celebrated Berkeley. In a pamphlet, published at St. Petersburgh in 1817, by Sir Alexander Crighton, some very interesting cases are recorded of the beneficial effects of the vapour of tar in pulmonary phthisis. Like creosote, however, and the kindred remedies, it seems only to act as a palliative.

The existence of creosote in *crude pyroligneous acid* has been already stated. M. Schweigger Seidel, and Dr. J. Davy, declare that, from their chemical examinations and experiments on animals, creosote is the base of the *Acqua Binelli*;[8] and from the way in which *empyreumatic water* is obtained, there can be no doubt but that the same remark holds true regarding it. It is procured by adding chalk, at a high temperature, to crude pyroligneous acid, till the effervescence ceases, and then drawing off, by distillation, a little more than half the liquor. These substances are well known to be excellent applications in skin diseases.

Soot is another substance which demands our attention. It was

[1] Pharmacopée de Lyons, p. 427.
[2] Lib. xxiv. 11.
[3] Lib. xvi. 21.
[4] Lib. xv. 7.
[5] Lib. xxiii. 50.
[6] On Skin Diseases, (1834) article Cancer.
[7] Bateman's Synopsis of Skin Diseases, p. 83.
[8] It is sold for 2*s.* 8*d.* an ounce in Italy.

at one time much used in this country, and various preparations of it were officinal in our pharmacopœias, of which the following may serve as examples:—

"*Soot drops. Fit drops. Tinctura fuliginis.*

Wood soot, ℥ij.
Kali ppm., ℔ss.
Sal. ammon., ℥i.
Aq. Fluvialis, ℔iij.
Digest for three days, and strain.
Antispasmodic."[1]

Soot drops. Tinctura fuliginis.

Wood soot, ℥ij.
As. fœt., ℥i.
Proof spirit, ℔ij.
Antispasmodic, ℥ss. to ℥jss. in hysteria.[2]

The *oleum fuliginis*, which was a carefully rectified oil of wood soot, was officinal in the London pharmacopœia.

Salmon, the empiric, already referred to, placed great reliance on various preparations of soot, in gout, palsy, and a variety of nervous diseases; and a compound tincture with rust of iron, he says, is "prevalent against the green-sickness in virgins, and all such like distempers attending them." The species of soot which he recommends, is "that of the smoak of wood, or wood soot, and of that kind, that which is thick, black, shining, and emplastic like pitch." "This," he says, "consists of an ethereal spirit, a piercing yellow oil, and a pure volatil salt."[3]

Recently, in France, M. Blaud has used preparations of soot with great success in the treatment of ulcers, and a variety of squamous and furfuraceous skin diseases. He has published two memoirs upon the subject, in the *Revue Médicale Française et Etrangère*, in which he gives various formulæ for preparing lotions and ointment with this substance, of which the following may serve as specimens:—

Lotion for ulcers, &c.

Olive oil, ℥xij.
Soot, ℥jv.

To be boiled for twenty-four hours with a gentle fire, and then strained.

Ointment for skin diseases, &c.

Soot, ℥ij.
Axonge, ℥i.

To be boiled for six hours over a gentle fire, then cooled, stirring thoroughly with the spatula, after which it is ready for use.

[1] Gray's Supplement to the Pharmacopœia, London, 1828, p. 324.
[2] Ibid. p. 358. *Vide*, also, the older London pharmacopœias.
[3] Salmon's Complete English Physician, p. 105.

M. Blaud states, that he has found from experience, that when the ingredients are boiled together, a much more efficacious remedy is obtained than when the mixture is made simply by combining the substances in the cold, which is to be accounted for by an additional quantity of creosote being generated by the action of the fire. When M. Blaud first published upon this subject, he believed that he was the first physician who had used soot, but he afterwards discovered that it had a place both in the French and British pharmacopœiæ.[1]

I have not found any allusion, in works of the materia medica, to the use of any preparations of *peat* in medicine, but I have been informed upon good authority, that the oil obtained by the destructive distillation of peat, after repeated rectifications, was used some years ago by a druggist in this city, in the preparation of an empirical carminative; and the same individual was in the habit of prescribing frictions and unctions with this substance in scabies and other skin diseases. And Dr. Rennie, in his Essays on Peat Moss, after stating that no living animal is ever found in it, says, that "to prevent mistake, it may be proper to observe, that it is not insinuated that moss water is in general an unwholesome beverage either to man or beast; on the contrary, it is generally safe, and sometimes salubrious. In certain diseases, it is supposed by some to be medicinal."[2]

The same individual who employed peat oil in skin diseases, and as a carminative, was also in the habit of distilling an oil from rags, which he used in toothache, and I believe that *rag oil* has been long a secret remedy in toothache. The idea may have been suggested to those who make use of it by the well-known oily substance procured by burning a cone of paper, being a very popular and a very successful application in toothache by the peasantry in various districts of Scotland, and which is called by them *paper oil*. When we reflect upon the composition of paper, it is easy to see that this paper oil must be very strongly charged with creosote.

The *animal oil*, so much extolled by Dippel and Hoffman, and the fame of which was so great on the continent, especially in Italy, is believed by Reichenbach to owe its virtues to creosote.

It is hoped that the details into which we have entered regarding the medicinal properties of those substances which contain creosote, and whose virtues have long since been discovered, will not be deemed mere matters of idle curiosity, and the time employed in collecting the materials considered as mis-spent, but that they will be regarded as furnishing a powerful argument in favour of the opinion that creosote is indeed a valuable medicine, and that its name ought no longer to be considered as a mere catchword, by which noisy empirics gull the public.[3]

[1] Revue Méd. Française et Etrangère, June 1834, and Jan. 1835.

[2] Rennie's Essays on Peat Moss. Edin. 1810, p. 563.

[3] However much the praises of the ignorant may have prevented scientific men in this country from justly appreciating creosote, they have created a great demand for it, and thus made it a most lucrative article both to the

It must not be thought that, in any thing which has been said, there is any wish to detract from the importance to medical men of the discovery of creosote. Far from it. None would venture to say that there has been no practical benefit reaped from the discovery of iodine, because burnt sponge, and various mineral waters containing that body, were known long before to be useful in discussing tumours, and curing bronchocele. In this respect, Courtois and Reichenbach are on a footing, for both of them have only presented to the medical world old friends unmasked.

CHAPTER II.

PHYSIOLOGICAL EFFECTS OF CREOSOTE.

Before we can rightly appreciate, or skilfully apply any therapeutic agent, it is absolutely necessary that we should be acquainted with the effects which it produces upon the animal economy, both in a state of health and disease; and it was under this impression that I made the following experiments upon the lower animals.

First Experiment.

The right jugular vein of an ordinary-sized terrier bitch was opened about the middle of the neck. It was intended that a dram of pure creosote should have been introduced into the vein, and with this view the pipette was charged with that quantity; but before two scruples had passed from the instrument, the respiration suddenly became hurried and sonorous, and the gentleman who was listening to the heart's action through the stethoscope, exclaimed, "*the heart has stopped*," and of course the farther introduction of the substance was discontinued. For some seconds after this, the animal continued to breathe hurriedly, and before expiring uttered a faint shrill cry.

Whenever respiration had ceased, the thorax was laid open. In performing this operation, it was observed that the muscles of voluntary motion contracted energetically when cut into. On exposing the heart, it was found to be perfectly quiescent; and its contractions could not be excited, either by pricking it with the scalpel, or making incisions into its substance. On the other hand, the œsophagus remained contractile for a considerable time; and,

manufacturer and the druggist. The extensive sale of a drug is, however, no argument in favour of its real value; for were any one to ascribe to creosote the wonderful properties which Morison says reside in his pills, of curing "*blushes, hurries, and of causing young ladies to enter a room gracefully, and of making every body happy*," and freeing them of all diseases that flesh is heir to, there would be few houses which would not smell of it.

on exposing the intestines, the vermicular motion was observed to be going on with great activity. The heart contained a considerable quantity of dark-coloured blood in all its cavities, but more in the right than in the left side. Its muscular fibre appeared flaccid. All the large veins leading to the heart were filled with partially coagulated blood, and those of the abdomen were also considerably distended. The lungs were dark coloured, from the quantity of blood which they contained; and in the bronchial tubes there was a considerable quantity of a reddish-brown, frothy, serous-looking fluid.

Second Experiment.

The femoral vein of a strong young terrier having been opened high in the thigh, twenty-five drops of pure creosote were injected. No sooner had this been done, than the sounds of the heart became inaudible, the respiration extremely embarrassed, and, after giving a faint cry, the dog expired in violent convulsions in less than half a minute after the introduction of the creosote. About a minute after the sounds of the heart had ceased to be audible through the stethoscope, but before respiration had entirely ceased, a momentary and faint fluttering was felt in the region of the heart.

Immediately after death the cavity of the chest was opened. The heart was observed to be quite quiescent, and its contractions could not be excited by making incisions into its texture; but when some of the blood with which it was gorged had been allowed to escape, it began to contract, and continued to do so for five minutes. The left ventricle contained arterial blood. There was observed, in this instance, but a slight trace of the frothy fluid seen in the former case. In other respects the appearances were quite similar.

Third Experiment.

Twenty-five drops of pure creosote were injected into the jugular vein of an ordinary-sized bull-terrier bitch. The effects produced were essentially similar to those described in the two preceding experiments.

Death ensued in this case some seconds sooner than when the poison was introduced into the femoral vein; and it is worthy of remark, that although the heart was perfectly quiescent when the thorax was opened, and did not contract when cuts were made into its substance, yet, when by means of an incision into one of its cavities some of the blood with which it was gorged had been allowed to escape, three distinct contractions were observed.

From what has been detailed, and from what I observed take place in the cases of several other dogs which I destroyed, both by introducing the substance into the stomach and the veins, the symptoms of poisoning with this substance in such cases may be thus described.

Its first deleterious action is the production of a powerfully sedative effect upon the heart; and, from the rapidity with which this is manifested, it may be said (without using the expression at all in a figurative manner) almost instantaneously to paralyse the vital energies of that organ. In some instances, hurried and sonorous respiration goes on for more than a minute after the heart ceases to beat. In general, one or two convulsive fits, approaching tetanic spasm, precede death, and almost invariably before expiring the animals utter one or more shrill cries.

In every instance the atony of the heart, immediately after death, was very striking. The voluntary muscles, œsophagus, and diaphragm, were uniformly found to contract when cut into; and in every instance the vermicular motion of the intestines was apparent. The only other phenomena observed in the examinations after death were more or less engorgement of the lungs, and, in most instances, the presence of a greater or less quantity of a reddish-brown, frothy fluid in the air passages and substance of the lungs. Neither of these appearances can be considered as at all peculiar, or what might not be expected in any case of sudden and violent death. The presence of the frothy fluid cannot be looked upon as in any way characteristic, as it is observed in various instances both of sudden and gradual death; and, in the cases under consideration, was not sufficient to warrant us to suppose that it was any specific effect of the creosote, but might arise from other circumstances attending the experiment.

From the following experiments, it appears that when creosote is thrown into arteries the deleterious effects produced are of a much milder nature; and that, if the dose is not pretty large, the animal will experience but little inconvenience.

Fourth Experiment.

Twelve drops of undiluted creosote were injected into the carotid artery of a terrier dog. No sooner had the substance passed into the vessel, than the animal gave a faint cry, but did not manifest any other symptoms of inconvenience for about a minute, when it became evidently affected. The breathing for a few seconds was loud and laboured, but this entirely disappeared. The heart's action did not seem to be at all impeded. For about twenty minutes the animal seemed affected with vertigo and stupor, but after that time this ceased to be the case; and in an hour and a half afterwards, when he was killed for another purpose, he was certainly not under the influence of the poison.

Fifth Experiment.

January 12th, 12 noon. About fifty drops of pure creosote were injected into the carotid artery of a rather large-sized sheep-dog. Before making the incision in the artery, it was tied below the place where it was intended to be opened, and as soon as the fluid had

been thrown in, it was secured by means of a ligature. No sooner was the operation over than the animal sprung on his feet, and made an almost successful attempt at escape from the room, the door of which was open. He was carefully observed for about an hour, but during all this time he exhibited not the slightest sign of any functional derangement. The edges of the wound were now brought together by means of stitches, and the animal allowed to remain at rest. He was repeatedly observed during the course of the day, and appeared uniformly quite lively. About 9 P. M. he was for the first time offered food, which he ate with great avidity. He had slight attacks of vomiting for two or three days, but this affection did not continue longer; and at the end of the fifth day, when he was killed, he was in good health. On dissection, the artery was found to be firmly plugged up with a coagulum of blood above and below the ligature, but with this exception there was nothing apparent which could be ascribed to the experiment.

Sixth Experiment.

A dram and a half of pure creosote was thrown into the carotid of a dog very similar, in size and weight, to the other two upon whom a similar experiment was performed. For about half an hour nothing was observed which could be traced to the injection of the creosote, although he certainly was slightly convulsed at intervals; but this was probably the effect of fear, as he had been similarly affected from the time he was secured for the operation, and even previously, owing to the manner in which he was confined. However, after this time, he became decidedly under the influence of the poison, and sank gradually into a state approaching coma, spasmodic twitchings of the whole body being pretty strong. For an hour he continued in this state, when he began to revive. He was then killed.

There seems, then, to be a very marked difference in the effects produced by introducing creosote into the arteries and veins. Not only are the bad consequences of injecting it into the arteries less apparent, but the last experiment would tend to show that this substance acts also as in the case of prussic acid, by producing coma, as well as by stopping the heart. That this opinion is well founded, appears more clearly from the experiments which next fall under consideration.

Seventh Experiment.

21st December, 1 P. M. Thirty drops of undiluted creosote were introduced into the back part of the mouth of a small young terrier dog, all of which was apparently swallowed. He immediately exhibited signs of uneasiness, and rubbed his head violently on the ground. In three minutes a quantity of saliva, white and considerably frothed, began to issue from the mouth, and continued

to flow for some time. In two minutes more, he fell down. In ten minutes after the poison was administered, the breathing became much laboured, slight spasmodic twitchings affected the whole body, especially the ears and extremities, and the heart's action appeared to be feeble and fluttering; but for some time no very accurate stethoscopic observations could be made on account of the convulsed state of the animal. In a quarter of an hour more, and in twenty-five minutes after the poison had been administered, the movements of the heart could not be felt, nor could its sounds be heard; but, on applying the ear to the chest, a loud bronchial *souffle* was detected, which continued to increase for some time, and then a rattling noise was heard in the throat. This lasted for a considerable time. Though the scalpel was pushed into the tongue, (which was hanging out, stiff, and cold,) legs, and other parts of the body, he gave no expression of pain. From a wound which had penetrated one of the brachial veins, a considerable quantity of blood issued, and from this time the dog showed evident signs of returning consciousness; the pupils became dilatable, which, for a long time previously, had not been the case; the eyes gradually regained their lustre, and, in ten minutes after the blood had begun to flow, he was observed to move his head slightly upwards. From this time the convulsions, which had almost ceased, began to return, and rapidly became more frequent. Twenty minutes after the first signs of returning life had been observed, and in exactly an hour and two minutes from the time the creosote was swallowed, he made an unsuccessful attempt to rise, and in a few minutes afterwards he crawled several feet. After this exertion, the breathing became again greatly laboured, and he lay for some time quite motionless. In an hour and twenty minutes from the time of the commencement of the recovery, he was able to walk about the room, but was greatly disinclined to move.

At 5 P. M. he refused to eat, and continued to exhibit the same disinclination to move. The breathing was almost natural.

On the 22d, the following day, he ate hardly any thing; seemed very sick and dull, and could not be induced to leave the fire. During this day he had some vomiting.

On the 23d, he ate and drank several times, but uniformly vomited afterwards—apparently all that he had taken.

On the 24th, the vomiting continued pretty much the same as on the preceding day, but he was decidedly more lively.

On the 25th and 26th, he vomited very little, and took a fair allowance of food. In a few days more the bad symptoms had wholly disappeared, and he has ever since enjoyed the most perfect health.

Eighth Experiment.

Twenty-five drops were administered to a strong, large-sized, bull-terrier dog, but the whole of this quantity was not swallowed. In two minutes, saliva was seen issuing copiously from his mouth.

In eight minutes more, he was affected with vertigo and slight tetanic spasms, and his eyes became fixed and dim. For about a quarter of an hour after this, he was sleepy-looking and exhibited great disinclination to move, and at intervals he was seized with convulsions. After this, however, he began to revive; and in an hour from the time the poison had been given he seemed to be little, if at all, under its influence.

At 5 P. M. he ate heartily, but vomited during the night.

On the following day he took little food, and what he did take he generally vomited immediately.

On the 23d and 24th, the symptoms were very similar, though decidedly modified; and on the 25th, the effects of the creosote could not be said to be at all apparent.

From the last two experiments, then, it appears that when creosote does not prove instantly fatal, it may produce very striking deleterious effects; but that if the animal survive these for a certain time, they wholly disappear. To the cases related, I may add those of six other dogs, to whom doses of about thirty drops were administered. In these instances the effects were in every essential point identical with those detailed in the seventh and eighth experiments. I was unfortunately obliged to destroy all these six dogs a few hours after the drug had been administered, and was thus deprived of an opportunity of making any observations upon the more remote effects of the poison.

In these last-mentioned experiments, then, it appears that the symptoms produced were dimness and fixation of the eyes, vertigo, and coma, clearly showing that it was sensation, and not the heart, which was chiefly affected.

The following experiment may be considered as proving, that, when the dose is sufficiently large, the heart may be affected, even although the substance has been introduced into the stomach.

Ninth Experiment.

Thirty drops of pure creosote were introduced, by means of an elastic gum tube, into the stomach of a small rabbit. Almost instantaneously the animal was strongly affected, the eyes appearing very prominent; and, after uttering three or four piercing cries, expired in a convulsed state a minute after the drug was administered.

The chest was instantly laid open. It was observed that the heart was still contracting, but that its movements were very tremulous and feeble. The right side was greatly congested, and became every instant more so. In three minutes and a half after the creosote was given the heart had ceased to beat, and was quite insensible to stimuli. The peristaltic motion of the intestines was extremely energetic, and the stomach was very sensible to irritation. This continued to be the case for a considerable time.

From all these experiments, the poisonous effects of creosote

appear to bear a very striking resemblance to those of prussic acid. Like the latter, creosote acts toxicologically, either by stopping the heart, or by directly affecting the brain alone—just as the dose may be larger or smaller, or according to the manner in which the substances are introduced into the system. Another very striking point of resemblance between prussic acid and creosote, is the remarkable and almost specific power which they both possess of arresting vomiting, especially if it be unconnected with organic disease.

It is a curious fact, that in large doses creosote seems to excite vomiting; and that, when given in such quantities, with a view to check it, no good, but evil, results from giving a large dose. Dr. Elliotson has stated this opinion,[1] and Dr. Shortt informed me that his experience confirmed it. Large doses of prussic acid, also, seem to produce vomiting, when the poison does not prove fatal too rapidly for this observation to be made.[2]

The vomiting, however, which ensued in the experiments here recorded, must probably be ascribed to the development of inflammatory action. That such inflammation is excited, is known to be the case by dissection. Corneliani has stated this fact, and Miguet found traces of inflammation in the whole extent of the mucous lining of the digestive canal, in one of the dogs which he poisoned with creosote, but which survived the dose some time.[3]

In a dog which I dissected, about thirty hours after a large but not fatal dose of creosote had been administered, distinct traces of inflammation were observed.

The remarkable similarity between the poisonous effects of prussic acid and creosote will be seen by comparing the details already gone into regarding the latter, with the following remarks of Dr. Christison on prussic acid.

"The most accurate and extensive experiments" (on poisoning with prussic acid) "are those of Emmert, published in 1805, those of Coullon in 1819, and those of Krimer in 1827. They found that when an animal is poisoned with a dose not quite sufficient to cause death, it is seized in one or two minutes with giddiness, weakness, and salivation, then with tetanic convulsions, and at last with gradually increasing insensibility; that, after lying in this state for some time, the insensibility goes off rapidly, and is succeeded by a few attacks of convulsions, and transient giddiness; and that the whole duration of such cases of poisoning sometimes does not exceed half an hour, but may extend to a whole day or more. When the dose is somewhat larger, the animal perishes either in tetanic convulsions or comatose." And again—"The pure acid, according to Magendie, exhausts the irritability of the heart and voluntary muscles so completely that they are insensible even to

[1] "More than two drops I have sometimes seen aggravate the sickness." Medico-Chirurgical Transactions of London, Vol. xix. p. 227.

[2] Dublin Medical and Surgical Journal. Effects of an excessive dose of hydrocyanic acid. Nov. 1835.

[3] Miguet sur la Créosote, p. 26.

the stimulus of galvanism. The diluted acid has not always this effect. In the experiments of Coullon, the heart and intestines contracted, and the voluntary muscles continued contractile after death, as usual. But Schubarth states that the heart is never contractile, although the intestines and voluntary muscles retain their contractility. The reason of these discrepant statements is, that a considerable difference really prevails in experiments conducted under circumstances apparently the same.[1] In eight experiments on cats and rabbits, with the pure acid, the heart contracted spontaneously, as well as under stimuli, for some time after death, except in the instance of the rabbit, killed with twenty-five grains, and one of the cats, killed by three drops applied to the tongue. In the last two, the pulsations of the heart ceased with the short fit of tetanus which preceded death; and in the rabbit, whose chest was laid open instantly after death, the heart was gorged, and its irritability utterly extinct."[2]

In animals poisoned with prussic acid, the lungs are generally found of a dark colour, and gorged with blood, appearances which were also observed in the experiments at present under consideration. In the dogs poisoned with creosote, the voluntary muscles never refused to contract, when cut into immediately after death; nor in any one case was the vermicular motion of the intestines absent. These phenomena, however, did not always manifest themselves in an equally marked manner; and I observed, that in those cases where death was produced most rapidly, the contractility of the muscles and the vermicular motion of the intestines were strongest, indicating that the paralysing power of the creosote had not had sufficient time to extend its influence beyond the heart. The correctness of this theory, however, is rendered problematical by the curious circumstance recorded by Magendie, regarding those animals in which the prussic acid took effect with such tremendous rapidity, that he describes them as dropping down as if struck by a cannon ball. In these cases, he tells us that the irritability of the heart, voluntary muscles, and intestines, was *wholly exhausted.*

The manner of treating a case of poisoning with creosote, ought evidently to be similar to that adopted for the counteraction of the effects of prussic acid, namely, the exhibition of ammonia, chlorine, and other stimuli. Probably chlorine may be an antidote to creosote, as when these two substances combine, a compound is formed which contains no creosote; but it yet remains to be determined by experiment what the physiological properties of this substance are,

[1] A proper investigation of the physiological effects of prussic acid is yet a desideratum; for, until the discrepant statements of experimenters are reconciled, we must not rest satisfied. I was present at an experiment, along with Dr. J. Reid and Dr. James Y. Simson, when *an ounce* of Scheele's acid was administered to a dog. Death did not ensue for about a minute; and other dogs of the same size were killed on that occasion as rapidly, by six drops of acid from the very same bottle! And it is particularly worthy of remark, that the contractility of the heart was in none of the cases much, if at all, impaired.

[2] Christison on Poisons, edit. 1835, p. 696.

or whether it exerts any influence at all upon the animal economy. Other antidotes might be suggested, but, as their value can only be determined by actual experiment, they may with propriety be passed over. If an animal, to whom a poisonous dose of creosote has been given, survive a certain time, recovery generally takes place; and this is another point of resemblance between the effects of prussic acid and creosote. The transient nature of the immediate deleterious effects of creosote suggests the propriety of resorting to artificial respiration.

The circumstance of the heart's contractions being excited by disengorging it of blood, when cutting into its texture produced no effect, has been already stated. It is important, as indicating that bleeding, provided absorption be not increased, is the first thing which should be had recourse to in cases of poisoning by creosote, and certain other powerful sedatives, as it appears that in this way the dormant energies of the heart may be roused, and its contractions, so excited, may be afterwards sustained by stimulating treatment. Being struck with the advantages which might result from such a mode of procedure, I performed the following experiment, which amply confirms the notion just stated.

Tenth Experiment.

Thirty drops of creosote were injected into the jugular vein of a large mastiff dog. The heart immediately ceased to beat, and the animal died in a terribly convulsed state. No sooner had the heart ceased to beat than a large opening was made in the jugular vein and a quantity of blood allowed to flow. Soon after a small quantity had been lost, the heart's action could be distinctly felt through the parietes of the chest. It was then exposed, and its contractions were observed to go on for several minutes.

In the account of the fourth experiment it is stated, that from the time one of the brachial veins was wounded the animal revived. The quantity of blood lost in this instance was comparatively small, and perhaps the stimulus of the wounds contributed as much to the recovery as the loss of blood, especially as the intervention of valves must have prevented any direct good result from unloading the right side of the heart, as takes place when the jugular is opened.

Besides the immediate curative measures which ought to be resorted to, means must be taken to subdue the inflammatory action which a large quantity of creosote never fails to excite in the alimentary organs.

A case could hardly occur in which the medical jurist would find any difficulty in deciding upon the cause of death, if called to examine the body of an individual poisoned with creosote, as the strong odour of the drug, which continues for weeks to be exhaled by every tissue, along with the appearances presented on dissection, would be considered as proof sufficient. It is stated by Miguet, that in a dog, which he killed with creosote, all the tissues, except the liver, smelled strongly of the substance. I never could satisfy myself

of the correctness of this assertion, for no sooner was the body opened than the peculiar odour of creosote pervaded the air so completely that no one present would venture to say whether or not the liver was exempt from the prevailing smell.

The two experiments upon dogs, with creosote, detailed by M. Miguet, seem to confirm those which have been here described, but they are not recorded with sufficient minuteness to render them of much value.

He does not state his opinion as to the mode in which creosote destroys animal life, but puts, in the form of query to the reader, the three following hypotheses. That it proves fatal from producing inflammation; coagulation of the blood; or by some peculiar special action on the nervous system.[1]

The notion that creosote proves fatal by coagulating the blood, is obviously incorrect; for in no case did I observe any appearance in the blood, immediately after death, similar to that produced by bringing creosote in contact with it, viz., the appearance of numerous white specks caused by the coagulation of the albumen; and, of course, whatever influence inflammatory action might have in the cases related by Miguet, when the animals survived some hours, in those instances in which the dogs were almost instantaneously killed by injecting creosote into the veins, no such cause can be admitted.

Since the preceding experiments were performed, there has been published, in the first number of the British and Foreign Medical Review, an account of the results of experiments and observations, on creosote, of Dr. Guiseppe Corneliani, Professor of Clinical Medicine in the Great Hospital in Pavia. I have not been able to procure any detailed narrative of his experiments, but merely the conclusions to which he came. He says:—

1. The internal use of an excessive dose of creosote produces immediate death without organic lesion.

2. The same thing occurs if it be applied to a large nervous branch, or if it be injected into the veins in minute quantities.

3. If the quantity be not sufficient to produce death, it causes a torpor of sensation and motion, particularly of the lower extremities, the heart, the diaphragm, and the organs of the external senses.

4. As sedatives increase its effects, stimulants would probably relieve them.

5. It produces, when taken internally, an irritation of the gastro-enteric mucous membrane.

6. Oil and mucilage, when combined with it, render it milder; but given with vinegar it acts more forcibly.

7. Few patients can bear more than two drops, four to six times in the space of twenty-four hours.

8. Animals poisoned by it, pass their urine immediately or soon after death.

9. Applied externally, it diminishes irritation, and acts as a desiccator.

[1] Miguet sur la Créosote, p. 28.

10. Applied to ulcers and wounds, it promotes cicatrisation.

11. When inhaled, it causes stupor.

12. It acts as a styptic, provided that the divided vessel be not a large one.

(The experiments were made on lambs, rabbits, dogs, &c.)—*Giornale delle Scienze Medico-Chirurgiche*, *No.* 8. *Febbrajo*, 1835. *Pavia.*[1]

The statement, that oil and mucilage, when combined with creosote, render it milder, is probably correct, and is an observation which may be of considerable importance in practice. That vinegar increases its activity, might be easily supposed from its great solubility in that fluid; but of the correctness of the fact as stated by Dr. Corneliani, there seems to be some doubt. In three comparative experiments which I made, with a view of ascertaining the justice of his conclusion, there was no apparent difference in the activity of creosote when given with acetic acid, or alone. From the different strength of the animals, and from the small number of the experiments which I performed, they cannot be considered as showing more than that the addition of acetic acid produces no very decided increase of activity.

The two following experiments would indicate that the addition of albumen causes creosote to act more forcibly than when given alone, or even with acetic acid. The two dogs upon whom the observations were made, were of an ordinary size, and seemed, from their striking similarity in every respect, to afford proper subjects for a comparative experiment.

Eleventh Experiment.

12th of January, twenty minutes before three o'clock, two drams of pure creosote were injected, by means of an œsophagus tube, into the stomach; and, almost immediately, the white of two eggs. In six minutes after the creosote had been given, he became suddenly affected with vertigo, and fell over several times. In another minute his breathing became laboured and sonorous, but this lasted only for two minutes, when the breathing was comparatively little affected, and could only be heard upon applying the ear to the chest, when a distinct, though not loud, blowing sound was detected. For a short time, however, his breathing was so loud at intervals as to be heard at a considerable distance. In eight minutes after the creosote was given, he was considerably affected with spasms, and in a minute and a half more fell over, and was never after able to rise. The convulsions soon became very violent, and continued so for twenty-five minutes—the animal, during all this time, being in a state of complete coma, from which he never recovered. It was remarked, that the spasms were general over the whole body, but that the posterior extremities and lower jaw were far most violently affected. Although the convul-

[1] British and Foreign Med. Rev., Jan. 1836, p. 265.

sions were most violent during the twenty-five minutes mentioned, yet they continued pretty strong for an hour more, when they became feeble, and occurred only at intervals. At half past five, the body felt cold, and at first it was supposed that every mark of vitality had ceased to be apparent, but, upon a close examination, slight twitchings of the lower jaw and posterior extremities were observed. In a few minutes these also ceased to be perceptible. The cavity of the chest was then laid open, when the heart was found to be much gorged, and there was great venous congestion every where. The ventricles were quiescent, and refused to contract when irritated with the scalpel, but the auricles contracted at intervals spontaneously, and their vermicular motion was very evident.

Twelfth Experiment.

12th of January, 3 P. M. Two drams of creosote, mixed with two drams of pretty strong acetic acid, were injected into the stomach of a dog very similar to that upon which the preceding experiment was performed.

For seventeen minutes he did not exhibit the slightest sign of uneasiness, but in another minute he appeared giddy, and shortly afterwards fell, but rose again immediately. He continued to rise and fall alternately for six minutes, after which time he rapidly became comatose and lay in this state, and was strongly convulsed. In this case, the lower jaw was not for a long time affected, and never was much so; the spasms of the posterior extremities and abdominal muscles were most apparent. About half past four, the breathing, for the first time, became laboured and sonorous. At a quarter past six, the spasms being much abated, the chest was laid open. The heart was much gorged. The auricles were observed contracting distinctly, but, from a tumultuous spasmodic state into which the body was thrown upon opening the chest, by which all the viscera were heaved to and fro, it could not be decided whether the ventricles were or were not contracting. The venæ cavæ, pulmonary veins, and aorta, having been tied, the heart was cut out of the body, and it was then seen that the auricles were contracting at intervals of three or four seconds with considerable force, but that the ventricles were quiescent. All the vessels formerly tied were now suddenly cut through; and in this way the heart was almost instantaneously drained of blood. This caused it to contract with extreme rapidity, and so forcibly, that at each impulse it seemed as if jumping from the plate on which it was placed.

That "animals poisoned with creosote pass their urine immediately, or soon after death," can hardly be ascribed to the specific action of any poison, for such an occurrence frequently takes place in cases of sudden death from any cause; and I had abundant opportunities of observing that it was by no means a universal occurrence, as Dr. Corneliani states it to be, in cases of poisoning with this substance. Corneliani's ninth and tenth observations

seem also to require some modification. He states, "that when externally applied it diminishes irritation." Now this is by no means always the case, for frequently it produces great pain and inflammation when applied to some ulcers; and I once saw a pretty extensive sore, resembling a burn in appearance, caused by the overflow of saliva, which ensued upon the application of creosote to a carious tooth.

In medicine, creosote may be used with great advantage as a sedative or anodyne. To produce such effects, it is given in diseases of the heart, pulmonary complaints, vomiting, and to allay the pain of cancer, &c. A patient, under Dr. Shortt's care, in the Royal Infirmary, afflicted with cancer of the stomach, derived relief from pain in ten minutes after taking a dose of fifteen drops. When its sedative or anodyne action is wished speedily, the object is best attained by inhaling its vapours. When creosote is persevered with, in small and gradually increased doses, it acts as a tonic. In general it does not affect the bowels; but in two cases, in which Dr. Shortt pushed its use to a considerable extent, diarrhœa, and, in one of the instances, decided dysentery, was produced.

The effects of creosote on the urinary organs is remarkably capricious. It sometimes augments the quantity of urine, and, in diabetes, has been found occasionally to diminish its specific gravity, as well as its quantity. On the other hand, Dr. Elliotson mentions a case in which a minim, given three times a day, caused micturition nine times in an hour. It occasionally produces strangury. In some cases, it has been observed to produce very remarkable changes in the colour of the urine, causing it occasionally to assume black and other hues. Like turpentine, and many other substances, it imparts to it its peculiar odour with great rapidity. Creosote is in some instances diaphoretic.

There exists great difference as to the quantity of creosote which patients are able to bear. Dr. Elliotson found that its average dose at first should not exceed one or two drops, which, according to circumstances, might be advantageously increased to six or ten. In some cases, unpleasant consequences followed the administration of the fraction of a drop; whereas, in the case of one lady, he increased the dose to forty drops, and in another case he gave ninety drops, in the course of a day, with impunity. In the case of the lady, the forty drops occasioned no bad effects; but the addition of a single drop to this quantity produced the usual unpleasant consequences which follow too large a dose of this substance, viz., vertigo, insensibility, and vomiting.[1]

When creosote is taken into the mouth, it occasions violent pain, which extends to the pharynx. The pain of the tongue is extremely acute, but seldom continues more than a few minutes. The lips retain the feeling of pain much longer. For a considerable time after the pain excited in the mouth has subsided, a strong

[1] Medico-Chirurgical Transactions, vol. xix. p. 220.

and disagreeable taste of smoke remains, and some individuals allege that they have continued to feel this for several days after using it for toothache.

Insects and fishes, when immersed in a solution of twelve drops of creosote in two ounces of water, die in about two minutes.

After one or two waterings with creosote water, plants fade and die. M. Miguet watered a young and healthy rose bush which had a number of buds, and a newly expanded flower, for seven days, with a solution of creosote, and did not observe any change in its appearance; but on the eighth day it began to droop, or as he expresses it, "*son aspect était triste.*"[1] He dropped some of the same solution upon the rose, which was of a beautiful bright red colour, when it assumed a pale, and then a yellow appearance; and he observed that the same changes were produced upon the green leaves.[2]

The effects of creosote upon animal and vegetable life, are quite analogous with those of the tarry substances generally, and of peat water.

In concluding this subject it is proper to state, that great care was taken, when conducting the experiments here detailed, to guard against every source of fallacy; and I trust that this has been pretty nearly attained—a result in a great measure dependent upon the valuable assistance which I received from several friends, but especially from Dr. John Reid, whose dexterity in conducting such operations is well known.

CHAPTER III.

MEDICINAL USES.

Most new remedies enjoy a short-lived reputation, however destitute they may be of the virtues ascribed to them. There are always a number of individuals of a sanguine temperament, who hail the discovery of every new or pretended medicine with delight; whose peculiar mental conformation prevents them from justly appreciating them; and to whom oft-repeated disappointments can never teach caution. There are others, again, who are apt to err on the opposite side, and who are ready to scoff at the mere mention of a new remedy, or who, at least, if they do not go quite so far as this, are contented to remain in a state of skepticism, without ever dreaming of investigating the subject. Hence it is, that we frequently find one part of the profession bestowing the most lavish praises upon a new remedy, while others are at the same time stoutly asserting its utter inefficacy. From the numerous fallacies to which experiments upon the body, both in a state

[1] Miguet, p. 23. [2] Ibid., p. 24.

of health and disease, are liable, we must have accounts of the results which a number of practitioners have obtained from the use of the remedy in those diseases in which it is reported to be useful, before we can attempt to draw any general conclusions; for no opinion in medicine can be received with any degree of satisfaction, which is not the result of a philosophical induction from numerous facts, ascertained by a variety of individuals.

Creosote, fortunately, has now been tried extensively in hospital and private practice, both on the continent and in this country, so that we have already, upon the whole, a sufficient number of cases before us, to enable us to draw such conclusions as ought to bear a very near approximation to the truth. And we trust, that ere long it will be generally admitted, that although creosote is not the universal remedy which some pretend, it is nevertheless a most valuable addition to the modern materia medica.

Toothache.

For the last two years, creosote has been a very fashionable remedy for toothache in this city, and in an immense number of cases a very successful one. Ever since its discovery it has been much employed on the continent in this affection.

Unless there be a cavity in the tooth, through which the substance can be applied to the nervous pulp, which is, in most instances, the seat of the pain, no good can be anticipated with any degree of certainty; but, at the same time, I am aware of cases in which there was no cavity whatever, and in which the complaint seemed purely rheumatic, where a solution of creosote, in water or alcohol, proved of eminent service.[1] Its indiscriminate application by some, in toothache, has led many to think more lightly of its virtues than they would have done, had they only seen it used in cases where it was fairly indicated. That one application, as has been stated, permanently cures the disease, is unquestionably not the case; but it appears to be preferable to any other known remedy, both as it relieves the pain more certainly and speedily, and insures its absence for a longer period. Having a severe fit of toothache about twelve months ago, proceeding from a carious tooth, I introduced some pure creosote into the cavity. This afforded me complete relief, after the transient pain caused by its introduction had subsided. Before this, I was generally attacked every damp day with toothache, and I repeatedly tried most of the common remedies without experiencing any good result; but for nine months afterwards I never had a twinge. At the end of this period, however, my old tormentor returned, when I again applied

[1] In irritation of the gums, the following wash has been used by Frémanger with great advantage.

Distilled water, two ounces.
Pure creosote, four drops.
Mix them. Bull. Gén. de Thérap., t. viii. p. 269.

the creosote, and since this second application I have had no recurrence of the pain. This is not an isolated case, but is similar to many with which I am acquainted. Creosote, *in disguise*, is a very old remedy in toothache; and, indeed, it seems to have been applied many hundred years ago, under different forms, to this disease, as has been already shown in a former part of this essay.

Several different explanations may be given of the manner in which creosote relieves the pain of toothache. 1st, It may be by destroying the nerve. I have somewhere seen this explanation objected to, upon the ground that were the nerve destroyed, the pain would never return. If *wholly* destroyed, it certainly never would; but it may be *partially* destroyed, for the pain often returns months after nitric acid has been applied, in which case it is more probable that the remedy acts as a caustic than by coagulating albumen. 2dly, The creosote may unite chemically with the albumen of the fluids which are always exuding from a carious tooth, and thus form a crust to protect the nervous pulp from the irritating action of the air; or 3dly, It may perhaps afford relief by stimulating the loaded vessels of the nerve, and causing them to contract and expel the blood with which they are surcharged. That it in any case arrests the diseased action of the tooth, is extremely problematical. I have seen cases in which its application completely cured the pain, but where the caries went on rapidly.

The best method of introducing creosote into the diseased tooth is with a fine camel's hair pencil. When this has been done, the cavity should be filled with cotton saturated with pure creosote—care being taken that there are no adhering drops by means of which the interior of the mouth might be rendered hot and painful, and in some individuals even blistered. It is of great importance previously to clean out thoroughly the cavity. Frémanger, in a paper in the Bulletin Général Thérapeutique, insists upon the observance of this rule. He says—"Pour obtenir un résultat favorable de l'usage de cette substance, il faut que la carie soit parfaitement nettoyée et séchée; sans cette précaution indispensable l'action du médicament se passe moins sur la partie malade de l'os et sur l'extrémité du fillet nerveux mis à nu, que sur les matières contenues dans la cavité de la dent malade."[1] The most careful attention can hardly prevent a minute portion of the substance from finding its way into the mouth, but this generally produces no other unpleasant effects than a slight burning sensation for some minutes at the tip of the tongue, and a hot feeling in the mouth, and frequently in the lips, caused by the copiously secreted saliva which overflows being mixed with a minute quantity of creosote.

Caries.

Reichenbach[2] has recorded two cases of what he calls caries, in

[1] Bulletin Général de Thérapeutique, t. viii., May 1835.

[2] Bulletin Général de Thérapeutique, t. viii., May 1835. It seems unnecessary to refer to the journals in which M. Reichenbach's cases appear,

which the disease was cured by the application of lotions of creosote and water; but as he has given so little information regarding these cases, and as his assertion that creosote is useful in this disease has not been sufficiently confirmed by later experimenters, there does not seem to be sufficient cause for our trusting to it in this malady, which surgeons have hitherto accounted so intractable.

M. Frémanger states, that he believes it arrests the progress of caries, and that it does so by combining with the calcareous salts of the bones, and forming a new combination, which, by its solubility, tends to disengage the areolar tissue and stop the ulceration at the proper point for the commencement of cicatrisation. Before we begin to form theories as to the mode in which remedies act, we ought to be provided with cases upon which to found our hypotheses; but Frémanger does not seem to have been so furnished, for he only mentions one case (excluding those of toothache) in which he tried this remedy. We shall dismiss the subject by stating the facts. A patient with scrofulous caries of the first and second phalangal bones of the index finger, complicated with a fistulous opening into the joint, who had for ten months been using, without any advantage, preparations of iodine and mercury, was speedily cured by creosote. It was, for the first ten days, introduced pure into the fistula by means of cotton, and afterwards an injection of five drops to the ounce of water was thrown in, for which was afterwards substituted the following ointment:—

Cerate, one ounce;
Oil of sweet almonds, one ounce;
Creosote, thirty minims.[1]

Burns and Scalds.

Reichenbach, among his first experiments with creosote, as a therapeutic agent, applied it to slight scalds, in which he found it of eminent service. Suppuration was arrested, and the sores healed rapidly.[2] In the treatment of burns, it has been used in France by Berthelot and Coupil.[3] They state that it has a remarkable tendency to cause the sores to cicatrise from the circumference to the centre, and thus prevent those irregular contractions which, in so many instances, produce permanent disfigurement. A crust, in the first instance, forms on the injured surface, which spontaneously separates in a few days. In this, as in most cases of the external application of creosote, suppuration is prevented from taking place, or, if it has commenced, this diseased action is arrested. It appears, then, to be an exceedingly valuable application to burns, as it prevents the two most usual bad effects of such injuries—viz., exten-

farther than to state that his papers may be found in most of the German, French, and British medical periodicals of the last four years, and the substance of them in a pamphlet published in Paris in 1835, by M. Miguet.

[1] Bulletin Général de Thérapeutique, 1833, p. 268.

[2] Ibid. 1833, p. 208.

[3] Ibid. Feb. 1834.

sive suppuration and contraction of the cicatrices. Berthelot makes use of what he calls *l'eau de créosote*, which is a solution of three or four drops of creosote in an ounce of water.

Recent Wounds—Power of arresting Hemorrhage.

Creosote possesses, in a high degree, the power of arresting hemorrhage from the capillaries; but it is only for the purpose of stopping bleeding that it ought to be applied to those wounds which it is desirable should heal by the first intention; and its use should be abandoned as soon as the bleeding is fairly subdued, as by uniting with the lymph effused it forms a substance which prevents adhesion taking place, in the same way that any other foreign body would do.

As to the efficacy of creosote in stopping hemorrhage from large vessels, there seems to be great disparity of opinion. Some assert that it is wholly ineffectual, while others ascribe to it in this respect almost miraculous powers, stating that on this account it must prove invaluable on the field of battle.

Dr. Hœring, of Neustadt, has made this point the subject of experiment on the lower animals. He found that the hemorrhage from large arteries and veins was arrested in a few minutes by the application of a dossil of lint soaked in a solution of two drops of pure creosote in a hundred of water.[1] Dr. Bichthauer, of Küngelsau, relates a case in which profuse bleeding from a leech-bite in a child was immediately arrested by a similar application, after all other measures had been found ineffectual, and the patient was rapidly sinking.[2]

The styptic powers of creosote have also been experimented upon at Munich. In an experiment performed there, the carotid artery of a dog was cut through, and the bleeding was stopped by applying creosote, and then pressing the vessel with the finger. The blood did not flow, and the wound healed in a few days without the application of a bandage. The editor of the Edinburgh Medical and Surgical Journal, after relating this experiment, exclaims, "Is this credible?"[3] It certainly appears to savour somewhat too strongly of the marvellous; but from the following circumstance, which occurred to myself, I am inclined to believe that the statement is not so highly coloured as some may imagine. When experimenting upon the poisonous effects of creosote, I had occasion to inject a quantity of pure creosote into the carotid artery of a dog. The effects produced on the animal passed away in about twenty minutes, and he revived from a state of great stupefaction, and crawled about; but, to my astonishment, no blood had all this time issued from the wound, not even previous to the development of the poisonous consequences of the experiment.

[1] Gazette Médicale de Paris, Dec. 1834.
Same memoir.
[3] Vol. xliii. p. 243.

Ulcers and Chancres.

In the treatment of ulcers of various kinds, creosote may be employed with advantage; but it is in the scrofulous, aphthous, phagedenic, and venereal kinds, that it has been found most useful. Coupil and Berthelot say that they have applied it with advantage in the treatment of varicose ulcers; but here it is probably far surpassed by the black wash, which may be almost considered as a specific application in this species of ulcer. Various cases of scrofulous ulceration successfully treated by it are to be found in the recent numbers of the Continental journals; and I had lately an opportunity of seeing a case of this kind treated by Dr. Shortt, in the Royal Infirmary, where an extensive scrofulous ulcer of the hip, after resisting a variety of treatment, at last yielded to creosote, and was ultimately completely cicatrised. In an aphthous state of the mouth, occasioned by mercury, it is said to be peculiarly serviceable; and during last winter I had an opportunity of seeing a case of this kind, in which the most salutary effects were produced by it.

In Magendie's Formulary of New Medicines, as improved by Gully, a case of aphthous ulceration of the mouth is mentioned, where the following wash was used every two hours:—

> Creosote, half a dram;
> Gum arabic, one ounce and a half;
> Camphor mixture, ten and a half ounces. Mix.[1]

After the second washing the slough separated, and the depressions in the mucous membrane (several of which were as large as half a sixpence) filled up with remarkable rapidity.

Chronic venereal ulcers, after resisting every variety of treatment, heal generally under the external application of creosote. Dr. William Cumming, of the H. E. I. C. S., when in Edinburgh last winter, had various opportunities of trying it in such cases, in his extensive practice among the poor, and informed me that, even in cases which appeared unpromising, from the bad habit of the patient and the great extent of the ulceration, cicatrisation was rapidly accomplished through its efficacy. Künchel has used it in syphilitic ulcers of the throat with great success. He tells us that two patients, with deep and extensive ulcers on the interior of the mouth and upon the tonsils, who had been under the influence of mercury, without any good effect, for five or six months, were cured by him with gargles of creosote water in six weeks. These gargles were used five times a day, and along with their use were continued small doses of the corrosive sublimate of mercury.[2] Thus he found that these sores, though they did not heal under the influence of mercury alone, speedily took on a healing action when the creosote was used along with it.

[1] Page 205.
[2] Bulletin Général Thérapeutique, tom. v. p. 311.

M. Lessère, who has also used it with much success in such affections, has published the following very interesting case. On the 10th September, 1833, a patient came to consult him under the following circumstances:—On the uvula, velum palati, and left tonsil, were seated deep ulcers, of a livid colour, with abrupt edges. The neighbouring tissues were in a state of intense inflammation, and he experienced great pain in deglutition. During December the inflammation was subdued by antiphlogistic measures, and the ulcers were touched from time to time with nitrate of silver. The ulceration still continued its ravages, destroying the uvula, velum palati, and affected tonsil. Diluent drinks, sarsaparilla, extract of parsley root, and, lastly, eighty mercurial pills with opium, were given; but they only served to aggravate the pain. During this treatment, the ulcers neither increased nor showed any inclination to heal. In the beginning of March, the water of creosote (*l'eau de crêosote*) was applied six or eight times to the ulcerated surface by means of a pledget of lint, and in fifteen days the cicatrisation was completed. From this case it appears that, when the use of mercury has been abandoned on account of its want of effect, creosote, unaided by it, may accomplish a cure; and as a farther confirmation of this fact, I may state that some of Dr. Cumming's cases formerly alluded to were of this nature. From a consideration, then, of the last mentioned facts, in connection with the cases of M. Künchel, it appears that creosote, either alone or aided by the constitutional effect of mercury, is an invaluable application to this class of sores, which, from various circumstances, are found in practice so frequently to resist all ordinary treatment.

In the application of creosote to ulcers, there are several facts which the practitioner should bear in mind. It is important to remember, that water only dissolves one eightieth part of creosote. If a small excess of creosote be present, it will not fall to the bottom, but will float on the surface in the form of minute globules; and when the lint to be applied to the surface is dipped in the lotion so formed, these globules adhere, and in this way a very different wash is placed on the sore from what was intended.

It is sometimes proper to apply pure creosote to ulcers, but in most instances the aqueous solution, of various strengths, will be found to answer better; and in very few cases, where the raw surface is extensive, ought pure creosote to be used, as too severe irritation is generally the result. There are circumstances, however, in which it appears that large quantities of undiluted creosote have been applied with decided advantage to ulcerated surfaces of considerable extent. An interesting case of this kind is recorded by Künchel.[1] The patient was a man of sixty-five years of age, who had been afflicted for fifteen years with a large ulcer in the ankle, accompanied with lardaceous degeneration, and attended by the most excruciating pain. In these circumstances, M. Künchel applied it to the whole extent of the disease with the best possible

[1] Bulletin Général de Thérapeutique, 1833, p. 313.

result, and without the excitement of any undue inflammatory action. When creosote is used pure, it rarely requires to be applied more than once; but if the application be repeated, it should be with caution, and at intervals, lest inflammation be unexpectedly produced. More or less inflammation always follows the application of pure creosote to a raw surface, and it continues, according to circumstances, from a few hours to several days. It is a good rule, therefore, not to repeat the application till this has subsided. At the beginning of the treatment, creosote, either pure or in the form of lotion, should be more copiously applied than afterwards; and as soon as a healthy granulating surface appears, it may with advantage be altogether discontinued, and some of the common lotions of the metallic salts substituted: for it is a generally admitted fact, that, in treating solutions of continuity, the most signal advantage frequently results from varying the lotion or dressing. Creosote may be applied to ulcers under various forms, and it is for the practitioner to consider which is the most advisable, according to the circumstances of the case he may have to treat. Probably, in general, the best method is to spread the lotion gently over the surface of the sore, by means of a camel's-hair pencil. It may also be used in the form of ointment, and, if the ulcer is irritable, this may be spread on the surface of a poultice. In addition to the formulary given in page 236, the following, which Dr. Shortt used in a case of phagedenic ulceration, and probably in other instances, is subjoined.

Creozoti, gtt. x.
Aceti, ʒij.
Aquæ fontis, ʒij.
Misce et fiat Lotio.[1]

To chancres creosote should be applied with a camel's-hair pencil; one or two applications are frequently sufficient, and more may do harm. M. Berthelot relates a case in which a large chancre on the *glans penis*, after resisting treatment with the nitrate of silver, rapidly healed when it was dressed with lint soaked in a solution of creosote in water.[2]

Cancer and Lupus.

This is a disease rarely if ever cured, being one of those opprobria of the surgical profession which has in all ages baffled the skill of the most distinguished practitioners, and for which the knife must be still considered the only remedy. Many substances have, from time to time, been brought before the profession as specific cures of this disease, each in its day being extravagantly eulogised by its patrons; but there is not at this moment any one article, or

[1] See MS. Journals of the Royal Infirmary, 1835, case of John Norris, tinsmith.

[2] Bulletin Général de Thérapeutique, Feb. 1834.

combination of articles, in the materia medica, to which the surgeon trusts for the accomplishment of a cure of cancer.

Creosote has been proposed as likely to supply this desideratum; and as it is possessed of properties peculiar to itself, the most sanguine expectations have been entertained by some of its efficiency, and the result of the experience of those who have employed it is such as ought to lead to farther trials; but, till a number of additional observations have been made, its real value in the treatment of cancer cannot be decided.

Reichenbach, Graefe, of Berlin, Cloquet, and others to be noticed, state that they have employed it with great advantage. Graefe, in his surgery, gives a case of very extensive cancer of the face and palate, which was much improved by creosote;[1] and M. Breschet announced to the Academy of Medicine in Paris, during last year, that he had employed this remedy in a case of cancerous ulceration of the nose, in the Hôtel-Dieu, with great benefit.

M. Téalier applied a saturated solution of creosote in water to an open cancer, situated on the breast of a woman, who was suffering most excruciating pain from it; and it was with a view of alleviating this that he employed the creosote. The result is interesting. No sooner had the solution come in contact with the ulcerated surface than the patient complained of an acute burning pain in the sore, shooting through the right side of the chest, and extending from the head to the very tips of the toes. This continued for an hour, after which the pain entirely ceased, and the patient enjoyed uninterrupted sleep for ten hours. Subsequent applications uniformly allayed the pain, and under its use the sore assumed a more satisfactory appearance. The same gentleman has used it in various affections of the neck of the uterus. In one case, he applied a mixture of one part of creosote to three of water to an ulcer in that situation. The pain which was immediately produced was of such a nature as to cause the woman to toss about in bed like one in convulsions. To alleviate her sufferings, he ordered injections of cold water, but the pain did not wholly leave her till the second day, when she was quite free of it, which had not been the case for two months before. The creosote was continued, but was afterwards used in a less concentrated form, and the patient was doing well when he reported the case to the *Société de Médecine* of Paris.[2] These cases are interesting, from the remarkable effects immediately produced, but are of little importance in enabling us to decide upon the value of creosote as a remedy in cancer, from their imperfect state. M. Marchal has published a case of cancer of the lip, in which he believes he accomplished a permanent cure by means of creosote. The ulcer had all the external appearance of cancer, and was attended with the lancinating pains so characteristic of that malady. Besides applying to its surface lint soaked in pure creosote, he occasionally touched it with caustic; and under

[1] Dictionnaire de Médecine. Article Cancer.
[2] Revue Médicale Française et Etrangère, Fev. 1834.

this treatment the sore cicatrised, and the lancinating pains ceased. M. Marchal suggests the probability of the caustic inducing the ulcer more readily to take on a healing action under the use of the creosote, but ascribes to the latter the chief merit of the cure.[1] He states that the application of the pure creosote occasioned at first very acute pain; and this is the most common occurrence, though it is by no means uniformly to be looked for. In consequence of reading the above case of M. Marchal, I was induced to try the effects of the application of pure creosote in lupus of the nose. The size of the affected part was rather less than half a sixpence, and had been very slowly increasing for about three years. Upon applying pure creosote to this surface, the patient experienced *no uneasiness*. For about a week it was dressed with lotions of creosote water, and from time to time touched with undiluted creosote. For a day or two no change seemed to take place, but after this the parts surrounding the sore became inflamed, and at the end of the week the ulcer was decidedly larger than previous to the application of the creosote; and, in consequence of the obvious injury done by the treatment, it was abandoned.

Professor Wolff, of Berlin, tried the effects of injections of creosote water into the vagina in two cases of cancer of the uterus. In both instances violent pain ensued, and one of the patients was obliged, on this account, after nine days, to refrain from employing it; and the other, after persevering in its use for twenty-six days, was compelled from a like cause to desist. In neither case was there any diminution of the secretion or of the metrorrhagia. One of the patients expired after violent metrorrhagia, but the other died more slowly.[2]

During last summer, I saw it tried in the clinical wards of the Surgical Hospital, by Professor Syme, in a dreadful case of lupus. The fetor of the discharge, which was before very great, seemed to be corrected, but besides this there was apparently no good effect produced. The case, however, was so very bad that no application could be expected to benefit it—the articulation of the lower jaw being exposed on one side, and an immense surface involved in the disease.

Cutaneous Diseases.

Creosote has been found useful in various affections of the skin, both when administered internally, and when used in the form of lotion. Dr. Elliotson has published several important cases illustrating the use of creosote in skin diseases. In a case of acune rosacea he accomplished a cure. The patient had been subject for seven years to the eruption, and was, at the same time, affected with headache, nervousness, thirst in the morning, and acid eructations. She continued under treatment for a month without any

[1] Gazette Médicale de Paris, Fev. 1835.

[2] Medicinische Zeitung vom vereine für Heilkunde in Preussen, 1834. No. 30.

marked improvement, when Dr. Elliotson determined upon trying creosote. The advantage which resulted was very striking, for in three days the eruption was evidently diminished. At first, she took only two minims, three times a day, but this quantity was gradually augmented to twenty minims (a dram, daily), the farther increase of the dose being prevented by the supervention of giddiness and trembling of the body. She was discharged at the end of seven months from the time she began to take the creosote, when the eruption was scarcely perceptible, and the dyspeptic symptoms entirely removed. This case is particularly valuable, as Dr. Elliotson put the real efficacy of the remedy to the test. For, imagining that part of the benefit resulting from her low diet, he ordered her to be put upon full allowance, when the cure continued to go on progressing in the same gradual but decided manner in which it had previously been doing.[1]

In a chronic pustular disease not remediable by antiphlogistic measures, Dr Elliotson states, that he never saw such advantage from any medicine before.[2] Dr. Copland has found a saturated solution in water answers well as a lotion in *porrigo favosa*;[3] but most of those who have employed creosote in skin diseases, seem to have ordered it in a much more diluted form.

On the continent, creosote has been extensively used as an external application in *scabies*, *ringworm*, *impetigo*, &c., by Reichenbach, Berthollet, and others. Professor Wolff, of Berlin, cured, in eight weeks, a case of impetigo sparsa of twenty-five years' standing, with fomentations of creosote water.[4]

It is a very valuable application to *chilblains*, and may be used either in the form of lotion or ointment. Dr. Hahn, of Stuttgart, says, that whether they be ulcerated or not, he accomplishes a cure in the course of a few days, by means of fomentations of water and creosote.[5] I have seen it tried in four cases, and in all it proved decidedly efficacious. A complete cure was effected in every instance by two or three applications of a mixture of equal parts of creosote and almond oil to the part, which was, at the same time, well rubbed with a smooth cork.

In mammary and infantile excoriations, and chaps from colds, creosote has been used by Reichenbach, Hœring, and Fichtbauer.[6]

Chronic Glanders. Power which Creosote has of counteracting the Fetor of Discharges.

Dr. Elliotson accomplished a cure, in the course of a few weeks, of two cases of chronic glanders, by the sedulous use of an injection

[1] Lancet, July 4, 1834, vol. ii. p. 459.
[2] Medico-Chirurgical Transactions of London, vol. xix. p. 237.
[3] Magendie's Formulary of New Remedies, with additions by Dr. J. M. Gully. London, 1835, p. 204.
[4] Medicinische Zeitung vom vereine für Heilkunde in Preussen, 1834. No. 30.
[5] Gazette Médicale de Paris. Dec. 1834.
[6] Ibid.

of a diluted solution of creosote up the affected nostril, combining the treatment in one of the instances with the internal use of the remedy. The strength of the injection employed was one drop of creosote to one ounce of water. In one of the cases, he ordered an additional drop of creosote, but as the patient complained of the strength of the injection, it was afterwards applied of the original strength. Both patients contracted the disease from the same horse, and were very similarly affected. Dr. Elliotson describes the symptoms in one of the cases in the following manner :—"A dull aching pain across the brows and in the eyeballs; his nose felt stuffed, and there was a considerable discharge of a thick, yellow, offensive matter from one of the nostrils. He had also a cough, and expectorated matter of a similar character to that discharged from the nose. His health had not been disturbed till about two days before admission into the hospital, when he lost his appetite, and was seized with a pain in the epigastric region. His fæces were of a colour similar to that of the discharge."[1] The offensive nature of the discharge was speedily corrected by the creosote, and when the patient was dismissed, it had ceased; the evacuations were natural; and he left the hospital quite well. In the other case, the medicine was also given internally, but it is not stated that the amendment was more striking, or the cure more complete.

The speed with which the fetor of the discharge was corrected, by means of the creosote, points out one very important use of this medicine. Mr. Syme's case of lupus, already related, shows the same thing; and, in certain cases, creosote must prove of great value to the surgeon, as an agent for counteracting the effluvia of discharges. All who have tried it seem satisfied that it possesses this property. When the contents of the intestines have been very offensive, Dr. Elliotson has found advantage from impregnating clysters with it; and he and others recommend it in mercurial fetor.[2]

Diseases of the Eye.

Coster has used creosote in chronic inflammation and suppuration of the eyelids; and Dr. Shortt tried it in the Royal Infirmary here, last summer, in a case of purulent ophthalmia. The facts of the case are as follows:—A man was admitted, on the 30th of June, on account of purulent ophthalmia. Nitrate of silver, collyria, scarifying, and other remedial measures, were had recourse to, without any decided good effect; and on the 14th of July a drop of pure creosote was introduced into the eye. On the 15th, we find the following report entered on the journals. "A drop of creosote was introduced into the left eye yesterday at four o'clock, which at first occasioned considerable pain, but which was almost entirely removed in a short time by hot fomentations. It produced,

[1] Lancet. June 20, 1833.
[2] Medico-Chirurgical Transactions of London, vol. xix. p. 234.

soon after its application, considerable vascularity of the conjunctiva and eyelids, with a slight degree of swelling of the upper palpebra. This morning the eye appears less irritable, and the eyelids more open; the opacity is much as yesterday, but the chemosis is much diminished.

"Repeat fomentations to eyes, and the solutions of caustic to the right eye."

There is no farther report as to the creosote, so that probably it was not again applied; and there is certainly nothing in the effects produced by it likely to call for its farther use. Indeed, the consequences were such as ought to prevent those who are aware of the facts just stated, from ever introducing pure creosote into the eye.

M. Sanson tried creosote in different kinds of ophthalmia, but never observed the disease at all modified by the treatment.[1]

Gonorrhœa.

Dr. Elliotson having heard that creosote had been used in gonorrhœa with advantage, and believing the account not improbable, from the fact of the turpentines having always had considerable reputation in this affection after the inflammatory stage was past, and knowing that creosote bears a strong analogy to them in many of its properties, he was induced to make a trial of it. He gave, at first, two drops in an ounce of water, and then increased the dose to four, six, and at last to eight minims.[2] No good resulted from its use. His experiment having, however, been made upon a female, it cannot be considered as settling the question as to the efficacy of creosote as a remedy in claps, for, from the difference of the seat of the disease in males and females, medicines given internally, though they frequently seem to benefit the former, are seldom, if ever, of the least advantage to females affected with vaginal discharges.

Dr. Hahn tried the effects of injection of creosote water in the advanced stages of gonorrhœa and gleet, but without any benefit. In some of the cases, it caused a return of the imflammatory symptoms, and in one instance produced swelled testicle.[3]

Menorrhagia.

Dr. Hauff, of Besigheim, has found injections of creosote water of great use in arresting this species of uterine hemorrhage. He has only, as far as I am aware, published one case in which he tried it; and others do not seem to have employed it in similar circumstances. Dr. Hauff's patient was a girl of twenty, who had been subject, for eighteen months, to irregularity in the return of the periods of menstruation, and also in their duration. Ultimately,

[1] Compte rendu des Séances de la Société de Médecine—Séance du 7 Mars, 1834.

[2] Lancet, December, 1835, vol. ii. p. 435.

[3] Gazette Médicale de Paris, Dec. 1834.

from the profusion of the discharge, her countenance became pale, though not sickly looking. Her pulse was slow and feeble, and all the secretions and excretions went on languidly. She did not complain of pain in her loins, or lower part of the abdomen, and there was no indication of leucorrhœa. The blood evacuated was sometimes in black coagulated masses, and at other times it was fluid, and of a brown appearance. Various vegetable and metallic astringents, ipecacuanha in nauseating doses, astringent injections, and plugging, had all been severally had recourse to without effect. Frequent injections of creosote water were now ordered daily, and pledgets, soaked in a similar solution, were introduced into the vagina. In a few days the flow of blood became less profuse, and after some weeks was very trivial; and the neck of the uterus, which had formerly been soft and spongy, regained its proper consistence, and the patient's colour and strength returned.[1]

Tumours and Excrescences of various kinds. Bubo—Condyloma, &c.

The external application of creosote has been tried, apparently with considerable advantage, in discussing various kinds of tumours. Reichenbach employed it in whitlow.

M. Martin Solon applied it with remarkable success to a venereal bubo. Leeches had been repeatedly applied, and poultices of linseed and rice, and also lotions and frictions, with iodine; but it went on to suppuration. He now directed the rice poultice to be moistened with creosote water, and to be renewed twice a day. On the second day, the swelling was less; on the fourth, fluctuation was not perceptible; and in four days more the bubo was entirely discussed.[2]

Dr. Heyfelder has removed excrescences from the anus in a fortnight, by means of creosote, after the failure of other remedies. Dr. Hahn says, that condylomatous growths generally disappear under the use of lotions of creosote water, and if they are obstinate, he employs pure creosote. They then desiccate, and fall off in crusts.[3]

Dr. Ure states that creosote has been tried at Berlin, by Fricke and others, in those cases of endermic condylomata where the desired local changes are tardy in manifesting themselves. He says, that it seemed only to be useful when they were small and of recent date, and implanted on a smooth lubricated surface, such as the internal portion of the nymphæ and prepuce. Fricke has tried it on a very extensive scale, and says that it not only removes condylomes, but prevents relapse.[4]

[1] Gazette Médicale de Paris, Dec. 1834.
[2] Bulletin Général de Thérapeutique, Dec. 1834.
[3] Gazette Médicale de Paris, Dec. 1834.
[4] Dr. Ure's Observations on the Venereal Practice of Berlin. Med. Gazette Feb. 1836.

Diabetes.

From the result of the trials of Professor Berndt and Dr. Elliotson, it appears that creosote is sometimes of use in the treatment of this strange disease, and may be even greatly instrumental in the accomplishment of a cure.

The following particulars of the case in which Professor Berndt employed this remedy, are to be found in the Lancet of July 18, 1835, being translated from Kleiner's Repertorium for January, 1835.

"The patient was a man, fifty years of age, ill for the last sixteen months; he passed daily seven Berlin quarts of urine, sweet to the taste and smell, containing a good deal of sugar, and of a troubled aspect; the patient was feeble, his appetite very great, and he was tormented by constant thirst; his sleep was disturbed by the frequent necessity of making water, but he had no hectic fever. The treatment was commenced by administering a vomitive, which brought away some acid-smelling stuff. Rollo's plan of treatment was then employed for some days, and ipecacuanha was given in small doses; this produced no good effect, and, instead of the ipecacuanha, eight drops of creosote were administered in the form of pill every day. The quantity of urine now excreted fell to three, two and a half, and two quarts per day. It appeared at first to contain a good deal of alkalies, particularly ammonia, and remained troubled. The dose of the creosote was gradually increased, and, after three weeks, Rollo's regimen was abandoned, on account of the disgust it excited in the patient. At this time, the urine gave the odour of horse's urine, contained less sugar, and exhibited the first traces of urea, though it continued still turbid. Under the common diet, and increased doses of creosote, the urine diminished to two or one quart and a half; it was occasionally clear, and gave an acid reaction; the quantity of sugar became daily less, and that of urea greater. After some time, the patient's state was evidently improved. He now took twenty-four drops a day, his appetite was good and his thirst much less, and the urine flowed at from one and a quarter to one quart and three quarters per day. In a few days more it assumed a natural colour, contained all the ingredients of normal urine, and ceased to give any trace of sugar."

Dr. Elliotson has tried creosote in four cases of diabetes, the particulars of which are subjoined. The first patient was a gentleman about sixty years of age, plethoric, and with a full pulse. He had suffered two attacks of palsy. He had been ill for four or five years, but his complaint was always worst in autumn, and least severe in spring. He made water twelve times a day, and thrice during the night. He stated that the quantity of urine was not more than four pints. It was found to contain a great deal of sugar. He was ordered creosote on the 13th of August, and on the 10th of September he was only making water six times in the day, and once during the night, and it contained but a slight trace

of sugar. After some time, however, the frequent micturition and thirst returned, but Dr. Elliotson had not an opportunity of examining the urine.

The second case is that of a young gentleman who was making twenty-four pints of urine a day, of specific gravity 1038. It was very frothy, and contained a large quantity of sugar. In this instance, the general health was much improved, but the disease was not alleviated.

The third patient was a gentleman about forty, who had laboured under the disease for six months. The quantity of his urine, in twenty-four hours, was twelve pints. It contained sugar, and its specific gravity was 1031. The skin was always moist. After taking the creosote for about a month, his general health was improved, and the urine diminished in quantity, but increased in specific gravity to 1037. The farther history of the case is not known, and the patient died in two months after the time the above observations were made.

The fourth patient was a young man in the North London Hospital, who took eighteen, and, latterly, twenty drops of creosote, three times a day. His health was improved, and his urine reduced in quantity from thirteen to seven pints; in specific gravity, from 1037 to 1030.[1]

Epilepsy.

Dr. Elliotson tried creosote in epilepsy. He found it sometimes do harm; and where it seemed beneficial at all, it only served to render the fits less frequent, and, in some instances, to produce a tranquilising effect.[2]

Neuralgia, and other forms of Nervous Excitability.

Dr. Elliotson, observing the soothing effects which creosote had on epileptic patients, resolved to make a trial of it in neuralgia, and other forms of nervous excitability.

The first case in which he tried it, was that of a young girl twelve years of age, who was subject to attacks of severe spasms in the abdomen, twitchings in the legs and arms, attended by severe pain in the lower part of the abdomen. At first, the attacks were irregular in their recurrence, but at length she was seized every morning in the manner above mentioned. The pain she suffered was excruciating, and her face was expressive of the most intense agony. She generally continued in this state during the whole day, and then fell into a comatose state, in which condition she lay till morning. Her bowels were in a state of habitual and obstinate constipation, and she made water only once in twenty-four hours; but both the urine and alvine discharges were natural in appearance. Every known remedy had been tried in the country, and

[1] Medico-Chirurgical Transactions of London, vol. xix. pp. 132–135.
[2] Same memoir, p. 221.

Dr. Elliotson despaired of being able to afford her any relief. He found that three grains of muriate of morphia alleviated her sufferings a little, but in so slight a degree that he discontinued it.

She was ordered a drop of creosote three times a day, and the dose was gradually increased to seven drops. In the course of a month she was completely cured of her disease, and left the hospital in perfect health, having also regained her flesh.

The second case was that of a man afflicted with neuralgia of the posterior dental and nasal twigs of the superior maxillary nerve, causing dreadful pain and sympathetic contortions of the features. On the 22d of August, he began to take creosote in doses of three minims thrice a day. The dose was afterwards increased to six minims; and on the 28th he was better for the first time. The dose of creosote was gradually increased to eighteen minims. The patient steadily improved, and in a short time was quite well. It is necessary, however, to state that he had been similarly affected three years previously, and recovered without any medical aid; but viewing the case in connection with others, and knowing the powerfully anodyne properties of creosote, there seems every reason to attribute the cure in this instance to the remedy employed; and Dr. Elliotson has found it equally successful in several very similar cases of neuralgia, but the particulars of which are not published. He does not pretend that it is an infallible cure in this class of diseases, but considers that it ought to have a high place among the other medicines which are found useful; and, from the cases just mentioned, it appears that when they all fail creosote may accomplish a cure. In such a disease as neuralgia, the offspring of so many different causes, no one remedy can be expected to prove a specific; and every individual case will probably be found to exhibit some peculiarity, requiring a corresponding modification in the treatment. Dr. Elliotson conceives that it is in rheumatic neuralgia, not inflammatory, that creosote is most successful.

Dr. Elliotson says—"In that morbid tenderness of the surface of the body which appears so nearly allied to neuralgia, and which so often occurs in females, he has not seen it of any use. In common hysteria, unconnected with inflammatory condition of any part, he has often seen it considerably lessen the disease; the more rare and strange forms of hysteria have yielded to it; and the morbid excitability of those who are called *nervous persons* he has frequently seen abated by it in a remarkable manner. In the latter description of persons, however, it is better to begin with no more than half a drop, as occasionally more at first produces excitement of the head. Palpitation, depending upon mere morbid excitability of the heart, has yielded to it far more than to other remedies."[1]

Power of arresting Vomiting—Its Efficacy as a Tonic.

Of all the various uses to which creosote has been applied in medicine, there is none where it has been found so important as in

[1] Medico-Chirurgical Transactions of London, vol. xix. p. 224.

vomiting from nervous excitability, and independent of any organic disease of the stomach. As a remedy in such cases it excels all other known medicines. This application of creosote was accidentally discovered by Dr. Elliotson, from observing that when given in cholera it arrested the vomiting, though the liquid stools and the fatal termination of the disease were not averted. Dr. Elliotson states that he has never seen it fail in arresting vomiting when it proceeded merely from functional derangement. Dr. Shortt, who has tried it in about a dozen of cases of this kind, has found it equally successful, and it has proved as efficient in the hands of Dr. A. T. Thomson, of London, Mr. Bodington, of Erdington in Warwickshire, and several other gentlemen who I am aware have used it in private practice, but who have not published the result of their experience. There does not seem to be any notice in the continental journals of creosote having been used by foreign practitioners as a remedy in vomiting.

Mr. Taylor, apothecary of the North London Hospital, has published a list of cases, in which Dr. Elliotson administered creosote in vomiting, of which the following is an abstract.

Case 1.—11th Nov. Colica pictonum.—The patient complained of spasmodic pain of the stomach, and vomited every thing she took. No signs of gastritis. Creosote, two minims; mucilage, a dram; water, one ounce: to be taken every four hours. 12th, the vomiting and pain quite gone. 14th, has vomited once. Dose of creosote increased to four minims. 15th, vomiting has not returned.

Case 2.—Colica pictonum.—All medicines were rejected till the creosote was given, after which this never took place.

Case 3.—Spasmodic pain in lower part of abdomen, with great constipation and vomiting. All medicines were rejected. Ordered a draught, containing two drops of creosote, before taking pills containing croton oil. She took three of them before the bowels were moved, and was discharged cured in a few days.

Case 4.—The patient could not retain pills containing croton oil, which she was ordered, unless she took two minims of creosote before each dose. This prevented the sickness, and the bowels were soon relieved.

Case 5.—Vomiting in a supposed case of painter's colic, which was relieved by doses of three drops of creosote every three hours.

Case 6.—Attacks of colic, attended with constipation and vomiting. The patient was distressed with constant sickness. He was ordered a drop of croton oil, and creosote according to circumstances, by which means the sickness was stopped very speedily.

Case 7.—An hysterical patient admitted November 1st, who complained of burning pain at the epigastrium, and a sense of heat extending up the œsophagus. She vomited every thing she took, and had occasionally regular paroxysms of hysteria. She had been ill for eighteen months, and attributed her illness to a moral cause. She was ordered three minims of creosote thrice a day, and colocynth to keep the bowels open. On the 2d, the sickness continued severe, but was a little relieved. The dose of creosote

was increased to six minims. On the 4th, the sickness and pain were better. On the 22d, with a view of relieving the hysteria, a dram of carbonate of iron three times a day was prescribed to be taken after the creosote, and with the same view the dose of creosote had been previously occasionally increased at intervals. She had, before coming into the hospital, attempted to take the iron, but it was uniformly rejected. In conjunction with the creosote, however, the stomach bore it very well. The dose of the iron was gradually increased to half an ounce, and that of the creosote to fifteen minims. She continued to improve, and was dismissed on the 22d of December, free of sickness, and in other respects almost well.

Case 8.—Hysteria.—The symptoms very similar to the former case. She was ordered a dram of carbonate of iron thrice a day, and a minim of creosote for the sickness when required. These doses were subsequently increased. The symptoms were all relieved, but the pain and sickness returned occasionally. The latter was always removed by the creosote, and it relieved the pain more than any other medicine.

Case 9.—Anasarca, ascites, and it was also supposed disease of several of the abdominal viscera.—The man was distressed with constant nausea and vomiting. He died twelve days after admission into the hospital, but had no vomiting during that time, and he continued to take the creosote up to the time of his death.

Case 10.—The patient had, for the last four years, vomited his food almost immediately after swallowing it, and latterly his medicines, with the exception of creosote, which was the last medicine he took before coming into the hospital. He appeared quite well, and did not complain of pain in the stomach, and there was but slight tenderness over the epigastrium. He was in the habit of knitting his brows occasionally, and complained of giddiness, but not of pain in the head. In this instance neither creosote nor prussic acid did the least good. At one time the patient took ninety drops of creosote in less than half a day, without the development of any bad symptom. It was at first ordered in doses of a drop.

Case 11.—Phthisis and diarrhœa of two years' standing.—The compound powder of kino having ceased to control the purging, sulphate of copper was ordered, which had to a certain extent the desired effect, but made the patient sick. To relieve this, he was ordered to have a draught, containing two minims of prussic acid, ten minutes before taking the sulphate of copper pills. These draughts at first removed the sickness, but having afterwards lost their influence, creosote was given, when the nausea was again subdued.

Case 12.—Chronic bronchitis, with symptoms of incipient phthisis.—The patient was ordered sulphate of iron as a tonic, which produced vomiting. Two drops of creosote were ordered every six hours. The first dose stopped the vomiting, and it did not return for three days, when, upon increasing the dose to four drops, it was permanently cured.

The particulars of the remaining cases are not published, but those detailed are sufficient to show the success with which creosote has been given in vomiting by Dr. Elliotson. In the same communication to the Lancet, from which the above cases are abridged, seven more are enumerated, in which it was administered to relieve nausea or vomiting caused by taking the hydriodate of potash. In all it proved perfectly successful.[1]

In a severe case of vomiting, apparently from arsenic, Dr. Elliotson states that he has known it succeed astonishingly; and in the limited number of cases of sea-sickness in which he had an opportunity of trying it the desired relief was attained. He also prescribed it with equal benefit in a case of vomiting from pregnancy.

I had lately an opportunity of trying it in a case of the last description, and the effect was most extraordinary. The patient was a woman about forty years of age, in the family way for the thirteenth time, and who had, during her three previous pregnancies, been troubled with fits of vomiting, occurring generally at stated periods two or three times a day; and she stated that on these three last occasions she had miscarried. Being informed of this case by a friend who had the charge of her, I requested permission to try creosote, to which he consented, stating at the same time that he did not believe it would be of the least use. She was ordered three drops of creosote, thrice a day, diffused in water by means of mucilage. She took the first dose about an hour before her usual attack of vomiting was expected, and since that time she *never vomited*, and rapidly improved in health. During the first day on which she took the medicine, she had slight nausea, but that also was entirely cured in the course of a few days, by persevering in the creosote without increasing the dose. There is every reason to anticipate that creosote will prove useful in vomiting caused by pregnancy, not only from the instances in which it has been tried with success, but from a consideration of the nature of the cases in which more extensive observation has established its efficacy. Should this hope be realised, a very great desideratum will be supplied; for at present this most distressing and very common symptom attendant on pregnancy is considered little, if at all, under the control of medicine.

Dr. Bodington has published two very interesting cases in which he administered creosote successfully in vomiting. The first he describes in the following manner:—"Mrs. S., a young married lady, the mother of one child, experienced a most difficult labour about two years since, the operation of 'turning' having been resorted to in consequence of an unusual presentation. She has since suffered from relaxation of the uterine system, irregular menstruation, hysteria, and constant sickness, and, on one or two occasions, from uterine hemorrhage; but the most distressing of her complaints has been an almost daily vomiting of food, some hours after taking it, at intervals of two, four, six, or eight hours—the

[1] Lancet, August 15th, 1835, vol. ii. p. 625.

food always returning in an undigested mass, whatever length of time may have elapsed from the period of taking the meal. Thus the dinner, taken at two o'clock P. M., would frequently remain on the stomach until four A. M., and then be rejected in the same state as when taken, the only change being that produced by mastication. She complained also of a sensation of tightness and constriction across the chest, headache, and so much dimness of vision as to amount almost to blindness; her countenance looked bloated and pale, the pulse was small and feeble, and she had a constant feeling of weariness and prostration of strength. She had been for some months under medical treatment without benefit. She visited Cheltenham in the course of the last summer, and consulted some of the most eminent medical men there, with no better result."[1]

She again came under Dr. Bodington's care, when he prescribed a mixture of six drops of creosote and a little mucilage in six ounces of water. After taking two large table-spoonfuls of this mixture, a remarkable change for the better took place, and she had no sickness for a whole day and night, which had not previously been the case for six months. The catamenia came on shortly afterwards in a more copious and natural flow, and all her bad symptoms seemed moderated, to such an extent, indeed, that Dr. Bodington describes her being cured as it were by magic. She having discontinued the use of the medicine on account of its nauseous flavour, the bad symptoms reappeared, when she was again prevailed upon to resume it—one drop being combined in the form of pill, with three grains of an aromatic confection. After a few days she was again relieved, and remained perfectly well.

The other case published by Dr. Bodington occurred in an infant eleven months old. The vomiting proceeded from suspension of the process of digestion, and had continued daily for a month, resisting all means employed to subdue it. The infant was completely cured after taking one drop of creosote in an ounce and a half mixture, a tea-spoonful having been given three times a day.[2]

From what has been stated, it appears that creosote possesses an almost specific curative effect in vomiting arising from nervous excitability of the stomach, produced by various causes; but there are cases of vomiting in which much mischief and no good will result from its exhibition. When there are any symptoms of gastritis, it should never be given. This is the opinion of Dr. Elliotson and Dr. A. T. Thomson. This latter has published a case of hysterical vomiting, complicated with inflammation of the stomach, in which decided injury was the result of the administration of creosote. It was not ordered by the doctor himself, but by the house apothecary, under the impression that it would relieve the hysterical symptoms, which it did to a certain extent. Dr. Thomson, in the course of his clinical lecture upon the case, made the following remarks to his students:—"You have heard that, during

[1] Lancet, January 30, 1836, vol. i. p. 694.
[2] Ibid., January 30, 1836, vol. i. p. 695.

the progress of the disease, symptoms of hysteria were experienced by our patient; and these are often the most distressing of the minor symptoms of gastritis. The globus hystericus is undoubtedly spasmodic, and consequently it was likely to yield to such a stimulant narcotic as the creosote; but it is important not to allow the relief, in this instance, to mislead you into an opinion that it may be given with impunity for removing those hysterical symptoms during the continuance of the active period of the disease. Nothing would prove more injurious, in my opinion, than a dose of creosote during the inflammatory stage of the disease; and there is much reason for thinking that it has again renewed the inflammation of the gastric surface."[1]

There are cases in which the stomach is in a peculiarly irritable state in hysteria, where the symptoms closely simulate those indicating gastritis, and yet there is no inflammation. As such, we must consider Dr. Elliotson's seventh and eighth cases. Dr. Roots, of St. Thomas's Hospital, also mentions a case of a similar kind. Believing it possible that there might be some insidious inflammation, he applied some leeches; but on the following day, his scruples being removed, he ordered her some creosote, which, on the dose being increased from one drop to three, speedily allayed the irritability of the stomach, which convinced Dr. Roots that it was not a case of gastritis; for he adds, "I never yet saw creosote taken into the stomach, where there was any thing like inflammation existing, without its producing either increase of vomiting or increase of pain, or heat in the stomach speedily after it had been taken. I was in fact confirmed in my original opinion, that this was a case of hysterical vomiting."[2] Dr. Elliotson says, that "where structural disease exists in the stomach, the diseased surface may not bear such a stimulant except in the minutest quantity; and a minute quantity, even should it arrest the vomiting, is very likely to aggravate pain."[3]

From the great extent of Dr. Elliotson's observations on the effects of creosote on the stomach, and from the deliberate manner in which he draws his conclusions, there can be little doubt of their usual accuracy. But the statement which he makes is not applicable to every case; for in Dr. Shortt's case of cancer of the stomach, already alluded to at page 230, the patient was relieved both of the pain and vomiting, by means of creosote. Perhaps, when disease of the stomach is suspected, it would be better to administer the creosote by means of inhalation. This may be done by breathing over a piece of paper steeped in it, or by inhaling its vapour mixed with that of water, from Mudge's inhaler.

In Dr. Elliotson's eleventh case, we find creosote stopping the vomiting after prussic acid had lost its power. Dr. Elliotson, who has written an interesting work upon the Effects of Prussic Acid in

[1] Lancet, Feb. 21, 1835, vol. ii. p. 744.
[2] Ibid., Dec. 26th, 1835, vol. ii. p. 492.
[3] Medico-Chirurgical Transactions of London, vol. xix. p. 227.

Affections of the Stomach, and has had ample opportunities of observing the comparative value of these two substances in the treatment of vomiting, believes that creosote has in a superior degree the property of enabling the stomach to bear medicines which have an emetic tendency—such as the sulphates of copper, zinc, and iron, hydriodate of potass, &c. &c. And in cases where both have failed separately, he has found advantage from their combination. Prussic acid is to be preferred to creosote in those cases where the patients have a strong dislike to the creosotic odour: this, however, may be almost entirely removed by prescribing it in the form of pill or bolus with an aromatic confection, as was done by Dr. Bodington. From the tendency which creosote has to act as a local stimulant, it is not so proper a remedy as prussic acid where there is disease of the stomach. It has this advantage, however, over the latter, of not being liable to the same uncertainty of activity, owing to the different strengths of which prussic acid is prepared, and from its liability to spontaneous decomposition.

Dr. John Walker, of Glasgow, has published a case which he says is "illustrative of the non-efficacy of creosote in allaying spasmodic or neuralgic pain of the stomach and vomiting, without inflammation of that organ." From the strain in which his communication is written, he seems to cast some degree of discredit upon the testimonies which Dr. Elliotson has given as to the power of creosote in such cases; but how an isolated observation can throw the remedy into discredit it is difficult to imagine, when its real value is attested in the fullest and most satisfactory manner by extensive trials in public hospitals, where the treatment is subject to the scrutiny and criticism of numerous individuals. Dr. Walker's case, then, can only be regarded as an instance of idiosyncrasy, and as such, from its extreme singularity in many respects, is peculiarly interesting. Dr. Walker describes the case in the following manner:—

"A. B., ætat. 22, an unmarried female. Has for the last three years complained of acute pain along the whole spinal column, greatly increased on motion or pressure, and accompanied by two lateral curvatures, describing, in all, the letter *S*, and including all the dorsal and lumbar vertebræ. There is no paraplegia; the uterus, bowels, urinary organs, &c. perform their functions regularly; the skin generally feels natural, and there is always some degree of appetite. Among the numerous symptoms under which this patient labours, the most distressing is constant vomiting of all ingesta, of whatever temperature or kind, and generally from two to twenty minutes after they have been taken. This vomiting of food is attended with considerable pain, but frequently violent retching takes place when no food has been taken, and which is attended by violent spasmodic pain of stomach, generally continuing from twelve to twenty-four hours after the retching has ceased.

"As every justifiable means had already been resorted to for the alleviation of the spinal affection (which in this case arises from

mere relaxation of the ligaments, &c.) without the slightest benefit, and the vomiting being not only the most troublesome symptom, but precluding all possibility of improving the general health, and as nothing had hitherto tended in the slightest degree to check it, I felt inclined to try the creosote. It was therefore given, according to Dr. Elliotson's plan, in doses of two drops in mucilage and water every six hours, but without effect. Next day it was increased to four drops, and in this quantity the first dose produced headache, and increased the vomiting and pain of stomach. The same quantity was again exhibited at the end of six hours, which only aggravated the former symptoms, and a short time after the administration of the third dose the headache became agonising; countenance flushed, with great heat of surface; retching and pain of stomach very severe. It also acted powerfully upon the bowels, producing six watery stools the first hour, with severe griping; likewise inducing painful micturition, and imparting to the urine a strong creosotic odour.

"The medicine was now intermitted for twenty-four hours, and again commenced in the same doses, but in combination with twenty drops of solution of mur. of morphia, when the symptoms induced were as violent as formerly, with the exception of the diarrhœa. Being still inclined to give the medicine a fair trial, it was again omitted for twelve hours, and afterwards exhibited when neither vomiting nor pain of stomach was present, and in doses of only one drop every six hours. The second dose produced violent and immediate vomiting, with severe pain of stomach."[1]

The most remarkable effects produced by the creosote were the watery stools, griping, and painful micturition; but the manner in which the bowels were acted upon is the most peculiar feature of the case. The headache, flushing of the countenance, and increased vomiting, are phenomena caused by the medicine in over doses; and it is possible that, had it been administered in extremely minute quantities, the desired result might have been obtained, for there is a very great diversity in individuals as to the dose they are able to bear. Dr. Elliotson states, that in some instances he has seen the fraction of a drop give rise to the physiological consequences just mentioned. Dr. Walker admits that, to a certain extent, the vomiting was caused by the affection of the spinal column and nerves. This case, then, must be looked upon as altogether peculiar; and to argue from it that creosote ought not to be exhibited in vomiting, independent of disease of the stomach, would be as unreasonable as to assert that mercury should never be used as an alterative, because cases occasionally occur in which a single small dose produces salivation and violent constitutional effects.

The non-efficacy of creosote, in Dr. Elliotson's tenth case, must also be referred to idiosyncrasy. Dr. Elliotson states that he has only seen two other such cases in which the vomiting was the disease and not the symptom, and in which creosote failed. In one of

[1] Lancet, December 19, 1835, vol. i. p. 447.

the instances he remarked that the patient, as in the case of the boy already mentioned, occasionally complained of giddiness and pain in the temples.

Pulmonary Diseases.

Creosote has been pretty extensively tried in affections of the lungs and air-passages. Reichenbach employed it in hemoptoe, and, if we can depend on the correctness of his statements, it is a useful remedy in this disease. A phthisical patient, who had for six days been spitting blood, and who was at the same time affected with fever, great pain, and difficult respiration, was cured in four days, by taking daily during that period five drops of creosote. In twenty-four hours after he began the use of the medicine, the spitting of blood was subdued, the pain ceased, respiration became more easy, and the fever disappeared. In another case of a similar nature, he accomplished as striking and as rapid a cure.

Creosote has been strongly recommended by Reichenbach and other continental physicians[1] in all the various forms and stages of consumption; but the more recent observations of Professor Wolff and Dr. Elliotson seem to point out, pretty clearly, that it is a remedy which ought not to be given in that disease. Wolff found that its use diminishes the quantity of urine, producing at the same time a greater disposition to colliquative sweats, and thus tending to complicate the original disease with dropsy. In one of his cases, the use of the creosote was discontinued on account of the violent vomiting which it produced.[2] Dr. Elliotson believes that, although creosote is no cure for tubercles, it may be useful when there are only one or two ulcers in the lungs, and no tendency to their farther promotion. He states, that through its agency a young gentleman, with a solitary cavity in his left lung, completely recovered, so that not the slightest morbid wound could be detected.[3]

In cases of increased secretion from the bronchial tubes, without inflammation, and in asthma depending on morbid excitability of the mucous membranes of the bronchiæ, it has been found of eminent service. I tried the effects of inhaling its vapour along with that of water, when lately labouring under a pretty severe catarrh. I think it tended to remove the uncasiness, which was before considerable. It certainly promoted expectoration, and unquestionably did me no harm.

[1] Bulletin Général de Thérapeutique, 1834, p. 384, &c. Miguet sur la Créosote.

[2] Medicinische Zeitung vom vereine für Heilkunde in Preussen, 1834, No. 30.

[3] Medico-Chirurgical Transactions of London, vol. xix. p. 221.

NOTE ON EMBALMING.

Having happened accidentally to look into the Asiatic Journal to-day, I was pleased and surprised to find there views regarding the embalmment of the Egyptians, briefly expressed by Colonel Bagnold, very similar to those stated in this Essay. Though he makes no allusion to creosote, yet, from the great community of ideas in other respects, it is necessary, to vindicate myself from plagiarism, to mention that my Essay was given in to the Harveian Society on the 1st of January, and that the number of the Asiatic Journal, from which the following passage is quoted, did not appear till February.

Proceedings of the Royal Asiatic Society, held 2d January, 1836.—Lieut.-Colonel M. E. Bagnold presented "a human hand, and a piece of beef, preserved by means of a preparation of vegetable tar found on the borders of the Red Sea, in the vicinity of Mocha; and a specimen of the tar.

"The secretary read a letter, referring to the above, addressed to him by Colonel Bagnold, from which the following passages are extracted:—

"'During my residence as political agent on the Red Sea, a conversation with some Bedouin Arabs, in the vicinity of Mocha, led me to suspect that the principal ingredient used by the ancient Egyptians, in the formation of mummies, was nothing more than the vegetable tar of those countries, called by the Arabs *katrân*. My first trials were on fowls and legs of mutton; and which, though in the month of July, and the thermometer ranging 94° in the shade, succeeded so much to my satisfaction, that I forwarded some to England; and have now the pleasure to send, for the society's information and inspection, a human hand prepared four years ago by my brother, Captain Thomas Bagnold. The best informed among the native Arabs think that large quantities of camphor, myrrh, aloes, and frankincense, were used; these specimens will, however, prove that such were by no means necessary, as the tar, when applied alone, penetrates and discolours the bone. The only use now made of this tar, in Arabia, is as a plaster or ointment for sore backs of horses and camels; rot in the feet of sheep; and, lastly, in the preparation of the heads of criminals sent from the distant provinces to the seat of government. The tar is obtained from the branches of a small tree, or shrub, exposed to a considerable degree of heat, and found in most parts of Syria and Arabia Felix.'

"The hand in question, though perfectly black, has little of the shriveled and distorted appearance of a mummy."—*Asiatic Journal for February*, 1836, p. 134.

March, 9, 1836.

APPENDIX.

No. I.—p. 214.

In Brosely, Bentley, Pitchford, and other places adjacent in Shropshire, there lies over most of the coal-pits or mines a stratum or layer of a blackish rock or stone, of some thickness, which is porous, and contains in it great quantities of bituminous matter.

The stone, being brought to the work-house, is ground small by horse-mills, such as are used for grinding flints to make glass of; the powder is thrown into great coppers of water, where, by boiling, the bituminous matter is separated from the stony or gritty—this last sinking to the bottom, the other swimming at the top of the water.

This bituminous substance, being gathered together and evaporated, comes to the consistence of pitch, and with the help of an oil distilled from the same stone, and mixed with the pitch, comes to be thinner, or like tar; the uses of both which materials, either for shipping or otherwise, these substances are said to supply, nay even go beyond. And this has been tried on several boats, this three or four years past, and does not crack as the ordinary pitch or tar, but always keeps black and soft, and therefore is proposed to hinder the worm from getting into the ships pitched with it.

There is likewise distilled from this stone an oil, which may be used for oil of petre or turpentine, and has been tried by divers persons in aches or pains.

In the valley called Liberthal, near Geesbach, (an ancient mine-work in Alsatia,) there runs out of a cavern a foul, fattish, oily liquor, which affords an excellent balsam, by taking a quantity of it and putting it in an earthen pot well luted, that no steam may exhale; and then, with a gentle fire at first, but a stronger afterwards, boiling it for three hours together, in which space it will boil in a fourth part, and an earthen matter like pitch will settle itself at the bottom; but on the top thereof, when cold, there will swim a fatty substance like line oil, limpid, and somewhat yellowish, which is to be decanted from the thick sediment, and then gently distilled in an alembic in arena, by which means there will come over two differing liquors—one phlegmatic, the other oily—which latter, swimming on the phlegm, is to be severed from it. The phlegm is used as an excellent resister and curer of all the

putrefactions of the lungs and liver, and it heals all foul wounds and ulcers. The oily part, being diluted with double its quantity of distilled vinegar, and brought three times over the helm, yields a rare balsam against all inward and outward corruptions, stinking ulcers, hereditary scurfs and scabs. It is also much used against apoplexies, palsies, consumptions, giddinesses, and headaches. Inwardly they take it with succory water against all corruptions of the lungs. It is a kind of petroleum, and contains no other mineral juice but that of sulphur, which seems to be thus distilled by nature under ground—the distillation of an oil out of sulphur, by art, not being so easy to perform.—*Abridgement of Phil. Trans.*, vol. ii. pp. 459, 460.

OBSERVATIONS

ON

DR. ABRAHAM COLLES'S WORK

"*On the Venereal Disease, and on the Use of Mercury.*"

BY RICHARD CARMICHAEL, M. R. I. A.

Honorary Member of the Royal Academy of Medicine of France, &c. &c., and Consulting Surgeon of the Richmond Surgical Hospital, &c. &c.[1]

Having reprinted in the pages of the "Library" the "Practical Observations on the Venereal Disease," by Dr. Colles, who has been led from observation and reflection to be a decided mercurialist, we esteem it advisable to reprint the following observations by an acknowledged friend of the author, who clearly shows—what, at the present day, is admitted by almost all therapeutists—that mercury can by no means be regarded as a "specific" in venereal diseases. Our own opinion is, that the large mass of syphilitic affections are susceptible of a cure without mercury; but still, that there are occasional cases which demand the use of an agent that is capable of exciting a powerful revulsion in the system, under which the morbid chain, that constitutes the disease when it has affected the constitution, is broken in upon, and if the revulsive effect of the mercury be sufficiently prolonged, the original disease is banished from the system. Mercury, in this view of the subject, being presumed to act in syphilis as it does in other long protracted affections, in which its advantages have been unequivocally displayed, not as a "specific," but like other agents that are capable of inducing a new condition in the organic actions; yet, as we have constantly affirmed, in no case has it appeared to us necessary to push the mercury to the extent that has been recommended by many.[2] R. D.

If the "Practical Observations on the Venereal Disease, and on the Use of Mercury," lately published, had come from the pen of a person less influential than that of my friend, Dr. A. Colles, I should have allowed them to pass without any remarks of mine; but his doctrines are so much in opposition to those which I espouse, that although he has not honoured me so far as to notice any of my published opinions on the subject, yet I feel that his observations

[1] Dublin Journal of Medical Science, No. 34, for Sept. 1837, p. 25.

[2] See "Intelligencer," for Nov. 1, 1837, p. 281.

have been in may places so *pointedly* opposed to them, that I should be deficient, not only in that just portion of *amour propre* which every man ought to possess, but in my duty towards the public, were I to allow his to pass unheeded, and to permit others to suppose that I had changed my mind, or acquiesced in doctrines which have not even the allurement of novelty to recommend them.

I trust that the observations I am about to make will be found devoid of all the acrimony which a desire to achieve a triumph is likely to induce, but that they will be made in that cool spirit of philosophical investigation, where the object alone is to ascertain a truth of considerable practical importance; and I beg particularly of the author, not to consider them in the light of an attack upon his opinions, but in that of a defence of my own.

The first passage, in the import of which I cannot agree, occurs in the first chapter, which treats on the "Natural History of the Venereal Disease." In it we find John Hunter eulogised in the terms he so amply deserves, as having ascertained many important facts relative to the natural history of the venereal disease not previously known. But then, as the author observes, he left much for his successors to achieve, and exclaims, "does it not appear strange, that subsequent writers have not made some efforts to supply those deficiencies?" He then proceeds to account for the little progress that has been made in this enquiry, as arising from the universal exhibition of mercury, which interferes with the natural progress of venereal complaints; for "every practitioner is so impressed," he observes, "with the sense of duty to do all in his power for the relief of his patient, that he cannot, consistently with that feeling, withhold such medicine, or refrain from such means as may best be adapted to the case."[1] Now this conscientious feeling has, no doubt, prevented our author from taking any steps towards ascertaining the natural progress of venereal complaints, when not interfered with; but as a feeling, perhaps as strong, has prevented others from having recourse to mercury in many forms of the venereal disease, so we must look to the observations of the latter, or to those who abstain altogether from the use of mercury, for information on the point in question. And I would here beg leave to ask, has no useful additional information in the natural history of those diseases been obtained since Hunter's time, by those practitioners who refrain from the use of mercury? Is not the classification alone of the various forms of the venereal disease, grounded on the nature of the eruption, a great and important step towards a just knowledge of their natural history? In Hunter's work, we find these eruptions all jumbled together under the general term of copper-coloured blotches, which is as little significant of their character as the term *dartre*, so generally applied by our neighbours to every variety of cutaneous disease. In Hunter on the Venereal, we find no mention of papulæ, pustules, tubercles, rupia, psoriasis, or lepra; all those forms of eruption, indicative of different forms

[1] Colles, p. 4.

of the disease, and often requiring different modes of treatment, are all classed under the one general and undistinguishable term of copper-coloured scaly blotches. Now Hunter is not to be accused of want of discrimination for this omission; because, at the time in which he lived, there was but little precise knowledge, and no accuracy of nomenclature respecting cutaneous eruptions. But when the profession became enlightened on this subject, chiefly by the labours of Dr. Willan, the information thus obtained was applied with manifest utility towards a just classification of the different forms of venereal disease.

That a classification, grounded on the character of the eruption, is not only in accordance with nature, but the most practically useful one which could be devised, is apparent from the following circumstances:—

1st. When a practitioner meets with a patient affected with a papular eruption, either in its early stage, when it presents itself in the form of pimples with accuminated heads, containing matter; or in its advanced stage, when the spots have desquamated, and present one of the appearances which Hunter calls copper-coloured blotches, he may be certain that he has only a disease of easy management to contend with, and that under suitable treatment he may assure his patient with confidence of a certain and speedy recovery.

2dly. If we meet with a pustular eruption which terminates in *superficial ulcers and not in desquamation*, we may be certain that we have a much more formidable disease to manage.

3dly. If there is offered to our consideration a case of pustular eruption, mixed with tubercles, which terminate in *deep ulcers which spread into a phagedænic margin, and form those crusts termed rupia*, we may be certain that we have the most unmanageable and destructive form of venereal disease to contend with. For under the most judicious mode of treatment, we too often fail in curing the malady, which annually destroys numbers, after enduring a lingering and loathsome catalogue of disgusting ailments. For it is in this form of the disease that we meet with extensive ulceration engaging the velum, uvula, tonsils, and back of the pharynx where it lies upon the bodies of the vertebræ, ulceration of the nares, destruction of the bones of the nose, and also ulceration of the larynx, the usual precursor of laryngeal phthisis.

4thly. The scaly eruption comprising psoriasis and lepra, is a form of the disease which is in general manageable; for, unlike the pustular and phagedænic forms of venereal, it yields with certainty and quickness to the influence of mercury. It may, however, be accompanied by deep ulceration of the tonsils, and very obstinate nodes; but whether ulceration of the larynx and of the nares attends it, is more than I can say, never having witnessed it. This form of the disease, which, from the term "copper-coloured scaly blotches," was probably most frequently met with in Hunter's time, is now comparatively seldom to be seen. It is often confounded with other forms of venereal eruption, because papulæ and pustules,

in their desquamating declining stage, when the virulence of the disease is considerably exhausted, assume a scaly, copper-coloured appearance; and even rupia evinces the same tendency, when the disease is on the wane. But I have never seen an instance (although Mr. Colles adverts to several) of "a papular or scaly eruption degenerating into rupia."

The tendency of all venereal eruptions is, as the disease is becoming exhausted, and is gradually yielding to the powers of the constitution to become scaly. And when in this state, no matter in what form the eruption commenced, I never saw mercury do harm, but always observed the most decided and quick amendment to follow its adoption,—a general fact which must be a great comfort to those who are either too indolent, or too skeptical, to think it of any consequence to pay attention to the precise character of the eruption.

From the facts above stated, I have a right to deny the justice of the sarcasm of our author, that all who treated of the venereal disease since Hunter's time, have added no additional information relative to its natural history. It is a most important practical point, and, coupled with the natural history of venereal complaints, to be able, by an accurate diagnosis, grounded on the character of the eruption, to prognosticate the probable event; to be able to state whether the patient will recover in a short time, or whether he may linger for years, and finally sink under the disease; and also from the nature of the eruption to be able to decide on the most appropriate mode of treatment.

I have not, in these observations on the natural history of venereal diseases, insisted upon the much disputed point of a particular form of primary ulcer being followed by a particular or corresponding form of eruption. And I have refrained from doing so, because, though my opinions remain unshaken on this head, and are supported by a host of observations, yet as it is still *sub judice*, I am unwilling to assume it as a fact established in the natural history of venereal diseases.

But notwithstanding this concession, and that Mr. Colles declares, "that after a long and careful observation, he had not been able to trace particular forms of eruption to particular forms of primary ulcers," I must also declare, that nothing is more decidedly connected in my mind, than the links which exist between gonorrhœa virulenta, a patchy excoriation of the glans and prepuce, and the simple fungous-looking ulcer without induration, with the secondary symptoms—papular eruption and all its concomitants, as laid down in my work on venereal diseases; and, again, the connection which exists between the primary phagedænic or sloughing ulcer, with venereal rupia and all its dreaded companions, is equally evident.

That the papular and phagedænic forms of venereal disease, as described in my work, combine in each a concatenation of symptoms both primary and secondary, totally dissimilar in appearance, in character, in duration, and in susceptibility of being acted upon by medicine, is obvious to any man even of a limited experience,

and must be acknowledged as such by every candid mind solicitous after truth.

Mr. Colles's reasons for dissenting from this doctrine, I shall give in his own words:—"Firstly," he says, "I have not unfrequently observed varieties of eruption exist together in the same individual; for example, I have seen small venereal lichen on the face, while a large form of papular eruption occupied the trunk and the extremities; sometimes, also, I have found spots of a pustular character scattered through a general crop of the papular eruptions. Secondly, I have noticed, as a very frequent occurrence, that when the first eruption has been removed, either by the use of mercury, or by other means, that the second crop has proved of a different kind; thus when the first eruption was of that small pimply kind which resembles measles, it has been succeeded by a papular eruption, and this again by a pustular crop. And, thirdly, by injudicious treatment; for example, by the excessive use of mercury in bad habits, any one other form of eruption may be made to degenerate into one which is most obstinate and severe, namely that of rupia."

To these observations I reply, that small papulæ on the face and large ones on the trunk, in the same individual, do not evince any difference in the nature of the eruption on these two parts, but only a difference in its intensity. The same objection might be urged against the identity of the itch, small-pox, or any other eruption, because it happens to be more severe in any one part of the body than on another. Some explanation is perhaps required with respect to the nomenclature of venereal eruptions. Papulæ have constantly accuminated heads containing matter, which some might call pustules; but the true diagnostic distinction between venereal papulæ and pustules is, that the former end in desquamation, the latter in superficial ulcers.

If a case presented itself in which pustules that terminated in ulcers were intermixed with papulæ, I would say the patient was afflicted with the pustular eruption; always designating it by the term which signified the most severe and dangerous spots which appear on the patient. For in the pustular eruption we often meet with papulæ, and in the phagedænic eruption we often see the rupia, which marks the disease, intermixed with both papulæ and pustules; but the first, in my judgment, indicates the true nature of the eruption.

In the same manner, in small-pox, we often observe papulæ intermixed with the pustules, which latter constitute the characteristic signs of the disease, for no person thinks of calling small-pox a papular, but a pustular, eruption.

With respect to the second objection, I may briefly observe, that the *tendency* of all venereal eruptions in reappearing, either a second, third, or fourth time, is to assume the scaly form. But although I have been many years attending to the natural history and the progress of this class of diseases, I cannot call to my recollection a single instance, even under "injudicious treatment, and the

excessive use of mercury," of a papular eruption being succeeded by a pustular crop, or this last by rupia.

The successive crops of both the papular and pustular eruptions have, as I have just said, an obvious tendency to assume the scaly character, in which they appear like copper-coloured blotches, and even rupia itself has the same tendency; but with this modification, that the spots exhibit the appearance of raised rounded tubercles, of a scaly dark red or copper colour, of which we possess many excellent delineations amongst the admirable collection of drawings belonging to the Richmond Surgical Hospital.

At page 76, I find the following observation,—"We shall now consider the proper treatment for a case of true venereal ulcer, the Hunterian chancre:—The local treatment should, in my opinion, be confined to the most bland and mild applications, such as cannot in any manner alter the features of the ulcer, for I am certain that many useful indications which may serve to guide us in the administration of mercury, are to be derived from observing the changes which these ulcers undergo through the agency of that medicine; these changes, and their corresponding indications, I shall hereafter advert to. I am further confirmed in this opinion from having observed that little or no benefit is derived from a contrary practice; thus, I have known a chancre completely cut out on the first or second day after its appearance, yet the occurrence of secondary symptoms was not prevented."

I cannot agree with my esteemed cotemporary in this reasoning or advice; for although *he* may have seen a chancre extirpated on the first or second day after its appearance, and yet secondary symptoms not prevented, yet I have seen, on the contrary, so many hundred instances of primary venereal ulcers treated by the free application of lunar caustic, with the view of destroying a surface secreting a morbid poison, which were not followed by constitutional symptoms, that I cannot but look upon this general exemption, by the mode of treatment mentioned, as a proof that I acted on a right principle. But, in order to cut off infection, the earlier it is attempted the better. We should also recollect, that during the first two or three days, and whilst the ulcer is still excavated, and secreting a thin ichorous discharge, it is in its most virulent state, and, therefore, most likely to infect the constitution. For we know, from the practice of inoculating vaccine and variolous matter, that the earlier the poison is taken, and before the matter becomes purulent, the more certain we are of imparting the poison;[1] and, therefore, it follows that the sooner we prevent an ulcer from secreting a highly infectious matter, the more likely are we to prevent the contamination of the system; and this is of more consequence than withholding our hand with the view of receiving "useful indications, which may serve to guide us in the administration of mercury."

[1] The satisfactory experiments of inoculation of venereal matter, by Mr. Evans, detailed at p. 80, second edition of my work on the venereal, establishes this view of the subject.

Concerning the prevalent exhibition of mercury, Dr. Colles observes: "In my opinion, nothing can more clearly establish the claims of mercury to be considered as a *specific* for this disease, than the frequent instances of cures made by it in the hands of those who must employ it at random; and very frequently on patients as careless and incautious as the prescriber is ignorant and pretending. It is wonderful that an engine so very powerful could have been so long wielded by the hands of the inexperienced, the injudicious, the uneducated and unprincipled, and yet should not have committed more havoc than it has done." But surely Dr. Colles does not mean to assert that mercury has not committed most dreadful havoc in the hands he alludes to. There is scarcely a day that we do not witness instances, either in public or in private practice, of the injurious consequences arising from the abuse of this most powerful medicine; but I will not limit this imputation to the "injudicious, the uneducated, and the unprincipled," for I assert, that as long as mercury is blindly administered even by the judicious, the educated, and the principled, under the deceitful and empirical notion that it is a "*specific*" for the cure of all venereal complaints, and not given as a medicine of unequivocal and ascertained powers, on sound pathological principles, we are not likely to improve medicine as a science, enhance our own characters, or what is of still more consequence, benefit our patients. Are we always to be swamped by the obsolete axiom, that mercury is the only appropriate cure for every form of venereal disease? Mr. Pearson formerly escaped from the many obstacles he met with in practice that militated against this notion, by christening those refractory symptoms that did not yield to mercury, syphiloidal, or sequelæ of syphilis; Mr. Mathias by calling them mercurial; Mr. Abernethy, pseudo-syphilitic. And now Doctor Colles tells us, that when mercury fails it is because it has been "injudiciously employed;" either in too small or too large quantities; "for too short or too long a time;" or that the patient has not been "duly prepared for its use;" or that he has taken it in a negligent manner, "and not under a sufficiently strict regimen." It seems *never* to enter into his mind that when it fails it may be *because it is an inappropriate or injurious medicine* for the form of disease under which the patient labours; or that it has been administered at an unfavourable period—for instance, during the continuance of the eruptive fever.

Thus we find the following observations with respect to the treatment of primary ulcers:—"Although I have repeatedly stated, that when ptyalism has been established, the further use of mercury becomes comparatively safe, yet it may happen, either from the mercury being continued too long, or too largely, in a weakly system, or from a state of fever induced by other causes, that a chancre which has made some progress towards healing, will take an unfavourable turn, and assume a phagedænic or a sloughing disposition."

Now here the ulcer, it seems, unluckily began to assume a pha-

gedænic or a sloughing disposition, "because the patient had taken mercury too long or too largely." But if our author had not been blinded by his prejudices, he would have said, because mercury is not an appropriate or fit remedy for any primary ulcer which shows a disposition either to the phagedænic or the sloughing process; but this would be expecting too much, for it would lead to an admission that there is a difference in primary ulcers indicating the necessity of different modes of treatment. Continuing his observations on these obdurate ulcers which so provokingly resist the infallible powers of mercury, we read at the next page but one, that "about twenty years ago, when I had charge of a large number of soldiers labouring under the venereal disease, who were received into the attic wards of Stevens's Hospital, I attempted the treatment of such cases by throwing in mercury largely and suddenly; but whether it was owing to the want of a judicious plan of using mercury, or to the bad habits of the men, induced by intemperance and dissipation, I know not; but I freely admit that with many this practice was not successful. However, the success of the two plans, that by mercury and that by the antiphlogistic regimen, was so evenly balanced at the time the military hospital was broken up, that I was quite undecided which to prefer. About this time I learned the use of the black wash, which has rendered such essential services in the early periods of this condition, that I have not since repeated the experiment of administering mercury through the constitution. Many of the cases treated with mercury were cured without the slightest destruction of any part, but this was purchased by the certain and severe sufferings of a violent salivation. Some escaped with the loss of part of the glans, and some few had the penis destroyed down to a level with the pubes. Possibly the mercurial treatment would have been the more frequently successful, had I more constantly used venesection and other evacuations as a process preparatory to the use of mercury. There is one condition of the sloughing penis which I look upon with total despair of being able to afford any means of arresting its progress until it has destroyed the entire penis down to the pubes: I mean that condition in which the sloughing part is so soft as to resemble melted tallow when beginning to form into a solid. I have never seen the progress of this arrested, even for a moment, by any local or constitutional means hitherto employed." And yet, at the very period alluded to by Doctor Colles, my work on venereal diseases was in the hands of every surgeon, and he would there have found a number of cases in which this form of ulcer was successfully treated, and the absolute necessity *emphatically* insisted on of abstaining altogether from the use of mercury. But I feel grateful for the candour of Dr. Colles's admission; and although I widely differ from him in my opinion, yet I have the utmost reliance on the honesty of his statements. Thus we find, that even in his hands mercury failed to benefit phagedænic ulcers; but surely he of all others should not attribute this failure either to a neglect of the preliminaries of "venesection and other evacuations;" or to an

"injudicious plan or mode of using it;" or even to the "bad habits of the men." For it is well known that soldiers being well fed, clothed and exercised, possess far superior constitutions to the generality of the inmates of hospitals. His reliance upon his favourite medicine, it seems, therefore, would have been sadly shaken, had he not at this time learned the use of black wash, the well-known combination of calomel and lime water. This fortunate discovery saved his confidence in the powers of mercury; and I believe, at the same time, not a few unworthy *members* of society from being reduced to mere stumps, if not uprooted altogether from their pelvic origins.

The good effects which so frequently arise from a spontaneous hemorrhage in stopping the progress of a phagedænic ulcer, is next adverted to. This fact I particularly stated in my work, (at p. 166, second edition,) and therefore recommended the removal of the jagged and uneven edges of such an ulcer by the knife, which induces a bleeding that succeeds often in checking its further progress. But as our author does not condescend to notice the works of any man since Hunter's time, I have no right to complain of his disregarding this, and some other points of practice, which I believe originated with myself.

In the next page, even after the ulcer is induced to heal, he recommends a course of mercury, with a view of protecting the constitution. Now I beg to protest against this practice, as I have frequently seen a phagedænic ulcer, after it had been healed, break out afresh, as soon as the mercury had affected the system, as if to evince how inimical is its use in this form of venereal disease.

In the same page, we find that a full course of mercury is recommended for the cure of chancrous excoriation,—a primary affection which I have mentioned as one of the causes of the papular eruption.

This recommendation, I acknowledge, astonished me; as I thought that such practice had been long since relinquished, having myself been in the habit of treating it with some mild astringent wash, and gentle aperients, during the last twenty-five years, a practice which usually removed the complaint in three or four days, the most obstinate case seldom extending to a week; and I found that constitutional symptoms as seldom occurred after this treatment, as when I had been in the habit of using mercury. On looking over the cases which Dr. Colles gives as illustrative of his mode of managing those ulcers, we find that Mr. W. used mercury in large quantities for the cure of this simple complaint, and that he even took five grains of blue pill, or three grains of calomel, three times a day, while he rubbed in mercurial ointment every night. The next case, (Mr. B.,) who was affected with so mild an excoriation, that he was treated with cold water at first, and afterwards was put on a mercurial course, which, however, did not prevent the accession of constitutional symptoms. He, however, it appears, got worse under the use of mercury, and it was laid aside in consequence of an excoriation occurring about the anus. The

patient then got well, and had not any return of his venereal symptoms.

Now, those who have been dosed with a full course of mercury know full well that it is really no joke, and it appears to me to be rather too serious a measure to inflict for a complaint, which, under the mild means pointed out, may be effectually cured in a few days; particularly as we find, by Dr. Colles's own showing, that mercury, however largely given, is not always successful in preventing the accession of constitutional symptoms.

Before I take my leave of his observations on primary ulcers, I must make some strictures on the following passage, which was obviously intended as a hit at my peccadilloes.

"I shall not attempt any further description of the various primary venereal ulcers which are daily to be met with; much less shall I undertake any classification of their endless varieties, or of the course with which each peculiar form has been supposed to run. They will be found to differ so constantly, that very rarely shall we find any two of them to correspond accurately with each other; not merely at their commencement do they present such dissimilar characters, but in their different stages towards healing they will be also found to deviate most strangely from each other, more particularly so when they have been treated by mercury."

Now, although I perfectly agree with our author that it would be impossible to describe the endless varieties of appearances which ulcers present, because every ulcer may be modified by age, constitution, mode of living, local and constitutional treatment, (particularly by irritating applications and mercury,) yet I contend there are at least four classes of venereal primary ulcers exhibiting, *ab initio*, distinct and characteristic appearances: 1st, the mild fungous-looking ulcer, devoid of induration or phagedæna; 2d, the ulcer with raised defined edges and smooth surface, without granulation; 3d, the phagedænic and sloughing ulcer; and 4th, the ulcer with a hardened base, or the true syphilitic chancre of Hunter. Now these four classes of ulcers may be so altered by the causes I have mentioned as to present interminable varieties; yet when we remove the inflammation, and wait until the mercurial irritation (the most common cause of these varieties) subsides, we will in almost all cases be able to recognise, in the ulcer under consideration, one or other of these four venereal classes.

We now come to the chapter which treats of venereal affections of the throat, and we find that our author attributes the great variety of ulcers in this part to the *manner* in which mercury has been employed. But it is better to let him speak for himself.

"In my judgment, the great varieties in the appearances and nature of venereal sore throats, are occasioned by the manner in which mercury has been used, either for the cure of primary, or for the treatmant of the secondary symptoms. No doubt, when mercury is used injudiciously, and in a manner unsuited to the general health and condition of a patient, we shall find that it causes more mischief, and produces more strange changes in the venereal ulcers

of the throat, if the patient be of a highly scrofulous, or of a very delicate habit, than if he be of a vigorous and healthy constitution."

Now this conclusion of our author is completely subverted by the facts which the anti-mercurial treatment has enabled us to observe; and therefore, as I before said, it is only amongst the anti-mercurialists that we can learn any thing certain about the natural history of the disease. For we meet with great varieties of venereal sore throats in those cases where not a grain of mercury had been used, and varying in mildness or severity in correspondence with that of the eruption which precedes or accompanies them. These have already been noticed in this paper, and sufficiently enlarged upon in my work on venereal diseases.

But I acknowledge that the injudicious use of mercury in cases unfitted for exhibition multiplies, *ad infinitum*, not only the variety but the severity of the ulceration. And when our author recommends a course of mercury for every venereal sore throat (for he does not acknowledge distinctions) "of eight or ten weeks' duration," we may readily conclude what the result must be in the papular or phagedænic form of the disease. In the first it is not only unnecessary and injurious, but in the latter such a sweeping recommendation is absolutely destructive.

The case of James Johnson, admitted Jan. 10th, is an excellent sample of the phagedænic venereal disease: rupia, and dark brown scabs on the forehead, eye-brows, and nose, and scattered over the entire body. "The arches of the palate, uvula, and tonsils, deeply ulcerated; the entire surface of the pharynx converted into, or covered by a soft slough, like half-boiled flummery, great emaciation, and weakness." This man was repeatedly salivated, before he was admitted into Stevens's Hospital, in other institutions. He was treated with sarsaparilla, nitrous acid, and lotions of nitrate of silver. On the 26th, ten grains of mercurial ointment were ordered to be rubbed in; the other medicines to be continued.

Feb. 6th. The ten grains of ointment were used *only every second night*, when diarrhœa set in, and the usual remedies for it were directed. We are not told whether the ointment was continued; but we find the ulcers were nearly healed; and "though he swallowed well, the fluid regurgitated through his nostrils whenever he drinks; and for ten days past he has complained of pains, &c., profuse night sweats, sleeps badly, appetite declining." The author then observes, "it is unnecessary to prosecute the details of this case farther." I believe not. And this is an instance selected as an illustration of the successful exhibition of mercury! Verily a man must have unbounded faith in his panacea, who could produce the treatment of this case as an inducement worthy of imitation. The amendment is attributed obviously to the ten grains of mercurial ointment rubbed in every night for ten nights, and afterwards only every second night, although, at the same time, sarsaparilla, nitrous acid, and strong local remedies were employed.

The case which follows, of an apothecary, who had a similar

ulcer of his throat, so deep and extensive as to cause exfoliation of the ring of the first vertebra, is as strong an illustration as I could wish of the ill effects of mercury in this form of the disease. But I shall not make farther observations upon these and many other cases equally to my purpose; but merely mention my surprise, that if a judicious use of mercury is capable of curing the phagedænic venereal disease, how it happens that we are seldom without some cases of this form of the malady in the Richmond Hospital, which had been previously many months in Stevens's Hospital, where, no doubt, mercury had been tried in the most "judicious manner." Now, I do not mention this by way of triumph, but as a fact, to show that mercury is not the appropriate remedy for this form of venereal disease; nay, that it is positively injurious and destructive.

In reply to this, I perhaps may be told that many patients are admitted into Stevens's from the Richmond Hospital—unsuccessful instances of the non-mercurial treatment. If so, I shall not feel in the slightest degree hurt by such a rejoinder; only premising this, that we of the Richmond are only so far non-mercurialists, that we do not give mercury in every form, or in every stage of venereal complaints, nor in any, except those few in which its use may be resorted to with advantage, as distinctly pointed out in my work.

In Chapter X. our author contends for the use of mercury in venereal hectic fever, and also in that fever which precedes and accompanies venereal eruptions. His words are: "I believe it is a pretty prevalent opinion at the present day, that when a venereal eruption is coming out, we should withhold mercury until the eruption be completed, lest we interfere with or interrupt that process. Another reason for withholding this medicine in such cases is, that the fever which accompanies the eruption is considered as unfriendly to the anti-venereal action of mercury. Now I am convinced, by repeated observations, that this rule not only may be departed from, but that it cannot be followed with advantage to the patient."

This doctrine is supported by some cases, in all of which mercury was given during the eruptive fever in minute doses; for instance, two or three grains of blue pill once or twice a day, or ten grains of ointment rubbed in every night, or only every second night, are the doses he prescribes. When he speaks of hectic fever, I presume he alludes to that fever which is an attendant upon those constitutional symptoms which harass and wear out a frame already nearly exhausted by useless efforts to overcome the disease. Now under such circumstances, if the eruption were scaly, and thus indicating that the disease was obviously on the decline, I should not feel the slightest objection to the cautious exhibition of mercury, having, however, first tried sarsaparilla in conjunction with the *hydriodate of potash*; which latter medicine I consider the most *powerful auxiliary* for the cure of the constitutional symptoms of the phagedænic disease, that has come into notice since I last published on the venereal. But as to the propriety of exciting mercurial action, whilst an eruption is

making its appearance, attended with an eruptive fever, I decidedly dissent, for the following reasons:

1st. Repeated observation has convinced me, that we lose instead of gaining time by doing so. For if the eruption is suddenly checked by the exhibition of mercury, cold, or any other cause, it will most probably return again and again in successive crops; a circumstance of which Dr. Colles must himself have met frequent instances, and which, I recollect, we witnessed more than once together in consultation, upon cases which *he* considered, at the time, very harassing and perplexing, as the patient had previously taken the *specific* in great abundance.

2dly. In suppressing the eruption suddenly by the exhibition of mercury, the patient is much more liable to nodes and affections of the deep-seated parts, than if a contrary practice had been followed; a fact which is strongly supported by the published reports of the surgeons of the British army on the non-mercurial treatment, by which it appears that nodes are much less frequent under the non-mercurial treatment;[1] and for these reasons I dissent altogether from the four conclusions which the author lays before his reader, in favour of the exhibition of mercury during the eruptive fever.

The cases detailed as illustrating the beneficial effects of mercury under those circumstances cannot be considered as affording any support to his position, for before the small doses given could affect the system, the eruptive fever had most probably declined. Our author himself seems, from the following passage, somewhat aware of the injurious effects of mercury during the eruptive fever:—"These two febrile states, viz. that of hectic and exhaustion, and that of the eruptive fever, require that the mercury should be administered in a manner peculiarly suited to such conditions. Were we to use mercury with these as we do with venereal patients in general, I believe we should commit most serious mischief. In these cases we should not commence with a larger dose than ten gr. of ungt. hydr. fort. every morning, or with an equivalent of blue pill; that is about gr. iij. mane nocteque."

And in the next page we find him stating that three or four drams of ointment are sufficient to produce the most salutary effects; for not only, he observes, "are the secondary symptoms dispersed by this small quantity, but the general health is proportionally improved, so that the patient rapidly acquires health and strength."

It is amusing to find that M. Devergie, chief surgeon of the Gros-Caillou, an advocate for the anti-mercurial treatment, exhibited the same minute doses of mercury as our author, but altogether under another motive. M. Devergie gave them *in finesse*, to satisfy such patients as imagined they could not be cured without that remedy: not that he had himself the slightest faith that such doses

[1] Connected with these observations, it is worth mentioning, that so early as 1819, the Army Medical Board, in its report on the non-mercurial treatment, states, that "Where the primary symptoms have been treated with mercury, the secondary symptoms are more severe, and more intractable, than where mercury had not been used for the primary sores."

would be of any advantage; but it is better to let him speak for himself.

"When, at the close of 1814, I became attached as senior surgeon to the Val de Grace, I was very desirous to make trials for arriving at the same result in the cure of recent venereal symptoms, by rejecting the employment of mercurial remedies. The principles explained in the works then published on the subject, and so accordant with the facts observed by myself, were assurances that I should succeed. But more than one obstacle prevented me from making conveniently, in the treatment of the venereal cases, the trials for which I was so anxious. Here, as every where else, mercury under three forms, ointment, liquor, and pills, formed in general the treatment of the primary affections. Physicians and patients showed themselves equally persuaded that this metal was the only remedy against these distempers. To propose publicly a reform would have been equivalent to provoking an anathema against me. I was therefore obliged to proceed with reserve.

"Nevertheless, from 1819 to the month of April, 1835, when I quitted the division of the venereal cases, aided by most of the pupils, I had several opportunities of introducing freely, in the treatment of this class of patients, the improvements suggested by experience and reasoning. Those who, already victims of the effects of mercury, refused to take more, when symptoms recently contracted brought them again to the hospital, fell naturally under my care. *As to those who believed that mercury was the true specific, I began mercurial frictions in small doses, one dram every two or three days, after the acute stage was over.* Already sparingly nutritive regimen, local blood-lettings, and rest, had effected an important modification. Time passed; the cure advanced; and *seven or eight drams* of mercurial ointment were sufficient to put these soldiers in a condition to quit the hospital after an abode of *thirty or thirty-five days.* Those, on the contrary, who were subjected to the ordinary mercurial treatment, used from *four to five* ounces of ointment, without counting the pills of Belloste (Ratier's Formulary, p. 123) taken in the interval between the frictions, and left the hospital only after fifty-five or sixty days. When the patients suffered themselves to be guided without anxiety as to the mode of treatment, I suppressed altogether the mercurial preparations; *and I arrived nevertheless at the same end; that is, of curing the patient more rapidly than by the ordinary method.* Instructed by so many observations agreeing with those afforded by my civil practice, I no longer doubted that *recent syphilis* might also be cured by simple and rational treatment. From this time mercury performed only a secondary part in my practice in the city. I soon renounced it almost entirely, and treated my patients by the antiphlogistic method."[1]

The remainder of M. Devergie's communication consists in giving the result of his anti-mercurial practice from 1814 to 1835, and

[1] Ed. Med. and Surg. Journal, from Archives Générales, October, 1835.

is chiefly composed of tables of the various classes of venereal symptoms thus cured, amounting to several thousands; to which I beg to refer the reader as a most important document. The difference between M. Devergie and Dr. Colles, in exhibiting these minute doses of mercury, appears to be this,—that the former deceived his patients for their advantage—the latter deceives himself to the advantage of no one.

In a work just published by Doctor Oppenheim, "On the Treatment of the Venereal Disease without mercury, together with a notice of the antiphlogistic treatment of the disease pursued in the General Hospital at Hamburg," we find that the author is a decided advocate for this mode of treatment, and was amongst the first who introduced it into the German hospitals.

"He believes that it can be shown from historical data, that since the period when mercury became to be universally esteemed the proper remedy for the venereal disease, the latter has increased in the obstinacy and violence of its symptoms; and he maintains, that when the treatment of it by antiphlogistic remedies alone shall be generally adopted, the beneficial effects produced upon the present as well as the future generation, will be scarcely less than those produced from the discovery of vaccination!

"Notwithstanding the foregoing facts and observations (all relating to the antiphlogistic mode of treatment) it has been almost universally believed that syphilis can be effectually cured only by specifics, at the head of which was ranked, and still is ranked by many, mercury. That numerous other remedies and plans of treatment have, at different periods, been proposed, will be seen from the previous chapters of this work. *Diet, depletion, and abstinence, have, indeed, always been considered as necessary adjuvants to insure even the efficacy of their presumed specific.*"

For this notice of Dr. Oppenheim's work, we are indebted to the American Journal of Medical Science, as it has not been as yet translated into the English language. From it we find a host of evidence in addition to that afforded by the surgeons of the British army, in favour of the antiphlogistic mode of treatment; and I would beg particularly to call the attention of Dr. Colles to the shrewd observations which I have marked in italics. I have now quoted the opinions and practice, recently published, of two most influential personages, the one in France and the other in Germany, directly opposed to those of our author, whose great object seems to be, to bring us back to the old orthodox faith—that there is no cure for a venereal disease but mercury; and that those modern heresies which have led some foolish people to recommend any other means, have induced incalculable mischief upon society. Now, although it seems, from various passages, that our author's faith remains unshaken in the "*specific*" powers of this mineral, yet he has discovered that one sixth, one twelfth, nay a twenty-fourth part of the doses he was formerly in the habit of prescribing, will prove as effectual a remedy as the original quantum; thus we find in many cases detailed, particularly of the phagedænic vene-

real disease, characterised by rupia, that ten grains of the ointment rubbed in every night, or every second night, or three grains of blue pill every night, or every second night, will be sufficient to cure the disease. I feel no objection to allow our author to ride his hobby as hard as he pleases, provided it produces no worse results than this practice. I know how strongly he is wedded to early opinions and associations, and that I might as well attempt to shake a pious Mussulman from his faith, that there is no God but Allah, and that Mahomed is his prophet, as to endeavour to dissuade my friend from his belief in the infallible powers of mercury over every form and every stage of venereal complaints—a belief which may aptly be parodied by a doctrine, the orthodoxy of which he will not dispute, that to overcome these maladies there is no God but Mercury, and Abraham is his prophet.

In Chapter XI., "On the Treatment of Syphilis in Scrofulous Patients," I find that Dr. Colles still perseveres in the notion that those enlarged cervical glands which occur for the first time, with the other symptoms of secondary venereal, are scrofulous. In my work on the former disease, I pointed out their occurrence as analogous and similar to that which takes place in other exanthemata; for all the different forms of the venereal appertain to this class of diseases. He asks, when these glands appear, "how are we to treat this complicated case?" and then he thus replies to himself: "my answer is, that we are to proceed exactly in the same manner as if the venereal symptoms were uncombined with any such affection;" —and my answer would be precisely the same, although I do not consider the case to be at all complicated—the swelling of the cervical glands having arisen from the venereal poison, and not from any scrofulous diathesis. But in a truly scrofulous subject, with a predisposition to tubercular phthisis, Hunter, in my opinion, was perfectly right in deprecating the use of mercury, particularly when given, as our author recommends in such cases, so as to excite a smart ptyalism. Several instances are adduced, however, of the success of the mercurial practice; but I rather imagine that the enlarged glands had, in those successful cases, a venereal origin.

Chapter XV. is dedicated to the consideration of the non-mercurial treatment of syphilis; of course our author is not likely to approve of it, but allows that "fewer of the non-mercurial patients complained of affections of the bones, than those who had been ineffectually treated by mercury." Such an admission from Dr. Colles cannot but be esteemed of value. He also "acknowledges that the profession is highly indebted to those who have lately introduced the non-mercurial plan of treatment, for we have not only acquired a second line of treatment for venereal cases, but, what is of the highest value, we have been released from an inveterate and deep-rooted error—from an unfounded conviction that the venereal disease could not be cured by the innate powers of the system, unless aided by mercury. I need not add, that all the opinions and practices consequent on this prejudice have been subverted."

Now, I agree perfectly with him as to the obligations which we owe to non-mercurialists; but I differ nearly as much in theory and practice from this class of practitioners as I do from the downright mercurialist. I look upon mercury as a most useful medicine for particular forms and particular stages of venereal diseases according to the system laid down in my work, which I have now pursued, both in private and public practice, upwards of twenty-five years, and never had occasion to regret my adoption of it. But the decided non-mercurialist, in his faults of omission, is perhaps equally erroneous, though not equally injurious to society, as the decided mercurialist, who with one fell swoop, and without compunction, blindly consigns every venereal case to his powerful, and, when thus indiscriminately employed, destructive *specific*.

At the conclusion of Dr. Colles's work, is an interesting chapter "On the Use of Mercury in Affections of the Nervous System," which he commences with the following observation: "Every surgeon who has been engaged in the practice of his profession during the last twenty years, cannot fail to have remarked the following fact; viz. that, during that period, mercury has been applied much more frequently to the cure of certain diseases than it formerly was, although its powers over these had not been previously acknowledged. How to account for the increasing partiality to this medicine, for the cure of other diseases, while its use in venereal complaints, for which it had so long been considered a specific, has within the same period of time been by many practitioners objected to, and by some totally abandoned, is a problem not easily to be solved." He then conjectures that the general application of this medicine to diseases not venereal, has been owing to the salutary effects which it evinced in the treatment of the Walcheren fever. I should rather attribute it to the well-known beneficial effects of mercury upon inflammation of the iris, for the first intimation of which we are indebted to Dr. Farre, in his letter to the late much lamented J. C. Saunders, on the disorganising effects of mercury, published upwards of twenty-five years ago. It was this first opened my eyes to the benefits likely to result from its application in all inflammatory complaints, and from that period I have been in the habit of employing it wherever I suspected the existence of internal inflammation.

In peritonitis, pleuritis, meningitis, periostitis, and lastly, in synovitis, its great utility is firmly established. In the last, or inflammation of the joints, my friend Dr. O'Beirne has put beyond a doubt, the vast advantages which result from a quick mercurialisation of the system, in a paper inserted in the fifth volume of the Dublin Medical and Surgical Journal, and in the fourth volume of the same publication, for September, 1833, I inserted a paper on "Inflammatory Affections of the Brain and its Membranes," containing six remarkable cases of head affections, including apoplexy and paralysis, five of which were treated successfully by bleeding, *mercurialising the system, and the counter-stimulus of tartar emetic ointment to the head.* The unsuccessful case, the first

detailed, would probably also have had a fortunate termination, were it not for the wilfulness of the patient, who discontinued the medicine and my attendance, at two distinct intervals, on finding, as he thought, sufficient relief and amendment from the means employed.

Amongst observations I made at the time on the cases detailed, is the following:—"In the treatment of inflammatory affections of the brain and its membranes, next to blood-letting, in efficacy, stands, in my opinion, mercurialisation of the system. The beneficial effects of this process in stopping the progress of inflammation of membranous parts, is most satisfactorily demonstrated every day by the exhibition of mercury for iritis. Frequently, in this affection, the pain, change of colour, and deposition of lymph on the iris, occasioned by the inflammation, begin to disappear, even before the mercury has had time to evince its usual effects on the gums of the patient."

Since that publication, I have met with a considerable number of cases of apoplexy, recent epilepsy, and paralysis, which have established, in my opinion, the value of this mode of treatment. In inflammation of the brain or its meninges, from accident, it affords our best safeguard in protecting the patient against the formation of matter and its other consequences; and this mode of treating all accidents of the head, in which internal inflammation is suspected, has been acted on with such success in the Richmond Hospital, during the last four years, as to insure a continuation of this practice. I have this moment, in conjunction with Mr. Kavanagh, under my care a young lady residing at Kingstown, who has recovered by this treatment from an attack of paraplegia, by which she had totally lost all power over the sphincter of the bladder, and the lower extremities. This young lady had been previously subject to most intense headaches, which sufficiently indicated the seat of the disease.

I therefore hailed with pleasure the confirmation of my opinions and practice contained in this section of Dr. Colles's work, in which are detailed some remarkable cases of hemiplegia, of confusion of the intellect, with tendency to apoplexy, gastrodynia, and epilepsy, treated successfully with mercury and tartarised antimonial ointment.

But, notwithstanding this general adoption of mercury for inflammation of membranous and parenchymatous tissues, I have been informed by some eminent apothecaries of this city, of long standing, that on a rough calculation there is not a tenth of the quantity of that medicine ordered now, compared to its employment twenty years ago; a fact which demonstrates the great diminution of mercury in the treatment of venereal complaints. Another fact is, I believe, also admitted by the seniors of the surgical profession, viz. that we do not now meet, in the same proportion as formerly, extreme cases of broken down constitutions from the combined effects of venereal and its presumed antidote. We have, therefore, reason to conclude that the treatment of the disease is, by the majority of

practitioners, better understood than it was twenty years ago; but it must be admitted that in general practice there is still great room for improvement.

Notwithstanding that Dr. Colles and I disagree upon many points, both in theory and practice, respecting venereal complaints, yet there are several upon which I give my unqualified assent. For instance, I agree with him, contrary to the opinion of Hunter, "that constitutional symptoms are capable of infecting and communicating the disease." In addition to the testimony adduced by Dr. Colles on this head, I have met with several instances of new married women, whose moral character sets them above suspicion, who received the disease from their husbands, who at the time of connection had no primary, but had secondary symptoms; and in no other way could I account for children before the age of puberty becoming diseased in consequence of lying in the same beds with persons labouring under constitutional eruptions or ulceration.

His rules for conducting a mercurial course, where such is advisable, are excellent, and his mode of fumigating by mercurial candles ingenious and suitable.

The chapter which treats of the venereal disease in infants, also contains much novel, useful, and interesting information.

His mode of treating a chancre at the orifice of the urethra with a strong escharotic, such as the white muriate of antimony, is decisive and most useful, particularly when the ulcer is of a phagedænic character; and when stricture of the orifice occurs after cicatrisation of the ulcer, his peculiar mode of treating the stricture is ingenious, and I make no doubt effectual. I also perfectly agree with him, contrary to the opinion of Hunter, that chancres sometimes occur in the urethra; but this was proved long since by the experiments detailed by Benjamin Bell.

Chapter VI. is on a "Disease of the Lymphatic Glands of the Groin attended with Peculiar Symptoms." In this affection one or more of the lymphatic glands of the lower or femoral range are generally the seat of the disease, which I have met with in many instances. It is always attendant upon a delicate constitution, and rapid pulse. In every case of this description, I have been led to suspect the existence, or at least a strong tendency to tubercular phthisis; and always directed my patients to the sea-side, and to observe those attentions calculated to improve the general health: and I perfectly agree with the author, that mercury in such a case would be injudicious and most probably destructive. On the whole, I am happy in stating my opinion, that this treatise contains many useful practical lessons; and that there is no surgeon, however extensive his practice may have been, who will not be benefited by a perusal of a work obviously the production of a man of extensive experience and undoubted talent for observation.

OBSERVATIONS

ON THE

TREATMENT OF TYPHOID FEVER BY PURGATIVES,

BY M. DE LARROQUE.

Report of Messrs. Louis, Bricheteau, Bouillaud, Double, Bailly, and Andral.

M. ANDRAL, REPORTER.[1]

We republish the following report on the employment of cathartics in typhoid fever, both on account of the interest of the subject, and the character of the commission from whom it has emanated. It must be observed, that the cathartics exhibited by M. de Larroque are generally of the mild kind—Seidlitz water, cream of tartar, calomel, and castor oil—and that they are given as "laxatives," not as perturbating purgatives. Of the propriety of the daily use of laxatives in an affection in which the intestinal secretions must necessarily be so much modified, there can be little doubt; but we at the same time think that there can be as little hesitation in rejecting the violent cathartics, which are often heedlessly exhibited in this as well as in other febrile diseases, in which quiet and absence from all irritation appear to be so obviously indicated.—*R. D.*

Gentlemen,—The commission employed by you to examine the work submitted to the Academy by M. de Larroque, on the treatment of typhoid fever by the evacuant plan, reports to you, through me, on the researches undertaken by that physician, and on the results to which he has been led. But, first of all, some explanations are due to you for the length of time which has elapsed between the period when M. de Larroque offered his work to the Academy, and the presentation of this report. Your commission had thought that, in a question so important as that whose discussion is connected with the exposition of facts which M. de Larroque has submitted to your judgment, it was for the interest of science that they should devote themselves to collecting facts, which they might compare with those that M. de Larroque has submitted to them, and which might serve them as means of control. It appeared to them that, if they undertook to decide by new experiments the question raised by M. de Larroque, it would be necessary

[1] Translated from the French by Mr. J. R. Atkinson, the editor's élève.

to appear before you, provided with such a number of facts as would obviate all chances of error that might arise, or at least be apprehended, from incomplete observation or precipitate judgment. Many members of the commission, therefore, devoted themselves to collecting facts, and the following is the plan they laid down. They were desirous to treat, at first, a hundred patients according to the plan proposed by M. de Larroque; then a hundred others according to the antiphlogistic method, and afterwards a hundred more on a plan nearly expectant. They proposed to analyse these facts, and to submit to you the results on which the merits of the work of M. de Larroque would have to be determined; but from such researches they could not decide at once. And as your commission has been frequently very earnestly called upon to present their report, they have, without abandoning it, postponed the work, the plan of which has been just pointed out; and to-day they submit to you their judgment on the memoir of M. de Larroque, as a provisional labour. Besides, they would not conceal the whole difficulty of such a subject: it comprises, indeed, and discusses the gravest questions, which for many years have agitated medical science, without having yet found one of those complete and definite solutions that compel and enchain the conviction of all. Thus is it not astonishing to see, that, whilst to many physicians the treatment of typhoid fever presents no other difficulties than in determining its seat or nature, to others, on the contrary, its therapeutics remains stamped with all the vagueness and uncertainty presented by its theory.

If, in fact, the pyrexia, now called in France typhoid fever, were nothing more than gastro-intestinal inflammation; if the numerous symptoms that occur in its course were only the sympathetic effects of primary irritation in the digestive canal, the therapeutic question would be one of the simplest—the antiphlogistic being the only method that could be had recourse to, differing only in its activity according to the particular circumstances capable of being recognised by the experience and tact of the practitioner.

If typhoid fever, having its origin in an inflammatory state of the digestive organs, presents likewise this remarkable fact, that the intestinal follicular apparatus is especially its seat; if, sooner or later after the appearance of lesion of the digestive tube, a certain set of symptoms break out—not only, as in the preceding theory, the sympathetic result of intestinal inflammation, but which are owing to putrid matters, deposited in the interior of the ulcerated intestines, becoming absorbed, and proceeding, like septic poisons, to infect the mass of blood, and consequently the whole economy; then the therapeutic problem presents a more complicated aspect. It may be conceived, in fact, that independently of the antiphlogistic treatment, which is always indicated throughout, we must endeavour either to evacuate the noxious and absorbable matters accumulated in the intestines, and then the utility of purgatives would be admitted; or to combat the infection of the blood, and for this end either chlorurets or tonics must be employed.

If, in fine, the intestinal lesion be regarded only as one of the elements, more or less indispensable to typhoid fever, which, differing only from true typhus by being less intense, attacks like it, or like small-pox, the entire organism, the therapeutic question then becomes again varied. Then, according to season and individuals, two kinds of problems are presented.

In one of these we search, in the midst of the general disorder, for some important modification, from which all the other phenomena might be supposed to arise; it is admitted that this modification is always identical, and consequently always opposed by the same treatment. Thus, with some, a great super-excitation of the blood-vessels is the predominant condition, and the debilitant plan is the only one that ought to be pursued; with others, on the contrary, an asthenic state, primary or secondary—to use the words of Brown—is the principal pathological condition, the major indication being to support the powers; and, accordingly, the tonic medication is the most effectual as well as the safest of all. With others, again, the principal part is played by a certain alteration in the liquids, by the presence of a morbid principle in the digestive tube, and even in the mass of blood; a noxious bile, which in the first place we must endeavour to expel; and the most useful method is that of evacuants.

Others lay down a problem entirely different—they ask themselves, if, independently of the intimate nature of the disease, which they do not endeavour to find out, the symptoms accompanying it do not present themselves sufficiently varied as to render it necessary to unite them into a certain number of groups, for each of which, by experience, one would be led to pursue a different treatment; they consequently admit an inflammatory, a bilious, and an adynamic form; each of these they recognise by the admirable features so well characterised and described by Pinel; to each form they adapt a special treatment; and they affirm that, according to age, temperament, constitution, and the infinite variety of influences, physical and moral, to which individuals have been exposed, and according to the epidemic genius prevailing at different periods, one description may become more common than another, and in some manner oblige the treatment to take as special a direction as the disease itself. They depict, in short, cases where, in the absence of symptoms or phenomena seeming to require active treatment, a simple expectant plan has been pursued with success. Cases of cure thus obtained, by serving to give rise to doubt as to the necessity of the employment of an active and perturbating treatment, show the applicability of the fine passage from Sydenham: "Natura enim sibi permissa negotium suum suo tempore exsequitur . . . ; ut nostrâ ope, nostris artificiis atque auxiliis, non indigent; suis viribus optimè instructa, suis opibus locuples, suo denique ingenio satis edocta."

Physicians of the most different schools, by pursuing one or other of the paths which I have pointed out, have treated the varied forms of pyrexia, now included under the generic appellation of

typhoid fever. During the last fifteen years, nearly all have, as you know, gentlemen, been exclusively treated by the antiphlogistic method, varying only by its different degrees of activity. Some physicians have always, in this lapse of time, protested against the universality of such practice, affirming that there are pyretic forms in which other treatment might be employed with great prospect of success; and, amongst those, emetics and purgatives have held the first rank. One of the wisest and most learned practitioners of the metropolis, whose recent and premature loss is still deplored by the Academy—M. Lherminier—very frequently had recourse to this treatment at La Charité; and the reporter to your commission, ever impressed with the honour of having been his pupil, has published the results of those researches, in a work on Clinics, which afforded some interest at a period when the administration of purgatives and emetics was generally proscribed. That was the commencement of a reaction, since regularly augmenting to the period when, adopting an old truth whose importance has been obscured by its abuse, it has been again acknowledged that, to expel emeto-cathartics from the domain of therapeutics, is to deprive ourselves, in a great number of diseases, of an immense resource; and that these agents are able to fulfil important indications which could not be accomplished by any other.

It is, in some measure, in the midst of this tendency to return to a certain order of ideas, too much abandoned, that Dr. de Larroque—generalising in his turn on the employment of the evacuant method, and not confining it to certain cases; pursuing it during the whole of the pyretic stage, and not making use of it in certain periods of its existence only; regarding neither difference of symptoms, nor lesions, nor even variety of individual predispositions—has established, as an invariable rule of treatment in every typhoid fever, the daily exhibition of an evacuant: in the first place, from one to two emetics; afterwards, the daily administration of purgatives from the commencement to the termination of the disease.

The work submitted to you by M. de Larroque, is divided into two distinct parts; one theoretical, the other purely empirical. In the first, he endeavours to point out that evacuants are constantly indicated in every typhoid fever, making the bile play a principal part in the production of this disease. "Morbidly accumulated in the intestines," says he, "it is a source of the most powerful irritation to the mucous membrane lining the digestive tube, and thus this membrane becomes inflamed, ulcerated, &c." If the most serious lesions are commonly met with towards the termination of the ileum, and in the cœcum, it is because in these parts of the digestive tube matters especially accumulate; their presence there may be easily proved by the existence of more or less gurgling, produced on exerting a degree of pressure on the ileo-cœcal region; if in a more or less advanced period of the disease these general phenomena make their appearance, which seem to be connected, on the one hand, with an alteration of the blood, and, on the

other, with a serious lesion of innervation, it is owing to the bile and other matters accumulated in the bowels being absorbed, and, like true poisons, infecting the mass of fluids, and consequently the whole economy.

These ideas, you know, gentlemen, were professed by Stoll, in the last century, and applied by him, not only to fevers which he called bilious, but to a great number of other diseases, and he hence deduced the utility of emetics, which were so frequently employed by him. This theory, once admitted, would perfectly justify the exhibition of evacuants generally in typhoid fever. But there is more than one objection to it; and, first of all, it has not yet been sufficiently shown that, in typhoid fever, the bile, reaching the intestine and remaining there, undergoes an alteration, and produces irritation, so as to determine there the serious disorders revealed on dissection. There are many diseases, during whose course patients remain for a length of time without having any alvine evacuations, and in whom, although we see some unpleasant effects from the prolonged retention, we observe the supervention of nothing analogous to what takes place in typhoid fever. It would then have to be admitted, that, in this last-affection, the bile, and other secretions into the intestines, possess peculiar acrid qualities, communicated to them by the disease; but this is precisely what has to be demonstrated, and for such a demonstration, the old experiments made by Morgagni on birds, which died soon after a certain quantity of bile, collected from the interior of the intestine of an individual who had died of a violent fever, had been injected into their blood, are insufficient. In our time, experiments of this description have been also made; but they have neither been sufficiently varied, nor frequently enough repeated, to enable us to deduce any consequences from them. Add to this, that, from the fourth or fifth day of typhoid fever, the intestinal follicles are observed to be swollen and diseased. Now, at so recent a period of the affection, when especially more or less diarrhœa has existed from the commencement, can one suppose that the bile has remained a sufficient length of time in the intestine to become altered, and produce in it those disorders? Lastly, it may be remarked, that in unfortunate cases, where, notwithstanding the daily administration of numerous purgatives, which have not permitted liquids accumulated in the intestines to remain there—that in cases, I say, thus treated, which have terminated fatally, as well-marked an alteration of the follicles has been found in the digestive tube, and one as far advanced, as in cases where, before the fatal termination, no evacuation had been solicited.

Thus, then, the theory of Stoll, who refers both the fever itself, and the most serious symptoms that accompany it, to the presence of bile in the intestine, and to the alteration it undergoes there, may be strongly contested; it is nothing more than a chance and hypothetical method of accounting for a certain order of facts; and it does not appear to us that it ought to be invoked as a justification

for the constant and invariable employment of emeto-cathartics in dothinenteritis. The theory might, indeed, find some support, were the success of the treatment incontestably proved.

Laying aside, then, every theoretical idea, which might, *a priori*, cast either favour or disgrace on the treatment proposed by M. de Larroque, let us examine into the facts themselves, abstraction being made of the more or less legitimate interpretation which we may be permitted to give of them, and let us judge them according to their results.

The different memoirs successively submitted to the Academy by M. de Larroque, contain the details of nearly a hundred cases, collected, under his inspection, by pupils attached to the practice of the Hospital Necker, and presenting nearly every possible form of typhoid fever, from that resembling a simple gastric derangement, or a slight enteritis, to that accompanied with the most violent ataxic or adynamic symptoms.

In all these cases, a uniform treatment was adopted. Convinced, indeed, that this is the only one capable of removing the cause of disease, and, consequently, of arresting its progress, M. de Larroque, notwithstanding the diversity of symptoms which may be presented, does not consider that there exists sufficient reason for varying the treatment which he employs. In the commencement, he administers from one to two grains of tartar emetic, whatever may be the form of the disease, and whatever appearance the tongue may present, whether dry or moist, red or pale, &c. The day after, he prescribes a bottle of Seidlitz water, and he repeats it during the continuance of the febrile stage. If the patient dislikes this kind of purgative, other laxatives, as cream of tartar, calomel, castor oil, &c., are substituted for it. Towards the termination of the disease, and when the febrile stage has nearly disappeared, M. de Larroque sets about recruiting the weakened powers; and, for the accomplishment of this purpose, he is in the habit of exhibiting tonics, and soon allows some nourishment. During the whole course of the disease, the usual drink is barley water and lemonade. Whatever symptoms may supervene, he does not modify the treatment, unless there be pulmonary engorgement, when he administers kermes mineral, which he gives daily in a linctus, in the dose of a few grains.

Whatever may be the form or symptoms of typhoid fever, bloodletting, M. de Larroque advances, ought *never* to be had recourse to. Not only, he says, is it in no case productive of advantage, but it is very injurious, and he affirms that the cases of typhoid fever observed by him to terminate fatally, were especially those which had been opposed at their commencement by more or less copious bleedings, although subsequently treated on the evacuant plan. Thus, with M. de Larroque, the treatment is a *unit*, as the nature of the disease is a *unit*, and this treatment has the greater chance of success, the sooner it is employed in the course of the disease, and when it is employed alone. Moreover, he relates cases in which evacuants were equally beneficial, when not

given until the typhoid fever had assumed a formidable aspect, and particularly when, although the patients were at a very advanced and dangerous stage of the affection, they had not been bled; which, according to M. de Larroque, places patients in the most unfavourable condition for cure. And yet M. de Larroque must be aware that many typhoid fevers have been treated by bleeding with a boldness which incontestable success justified; so that if the utility of blood-letting, seldom or often repeated, and to a greater or less extent, is not yet, for some minds, sufficiently demonstrated, its general and absolute proscription is not in accordance with the observation of all the facts.

M. de Larroque affirms, as a strict consequence of facts he has observed, and submitted in his memoir to the judgment of the Academy, that under the evacuant treatment, employed as just described, the different symptoms of typhoid fever disappeared in a safer and more rapid manner than under the operation of any other treatment; and those symptoms in particular were seen to give way, during their exhibition, which are considered, by many practitioners, to proclaim the existence of an inflammatory state of the digestive tube—such as redness and dryness of the tongue, thirst, meteorism and pain in the belly.

The influence exerted by the evacuant method, daily repeated, on the termination of the disease, is shown in a statistical table annexed by M. de Larroque to his work. Of a hundred individuals affected with typhoid fever, who are designated in this table, ninety were cured; ten only died, and of these ten, several had not commenced this treatment until they were in an almost hopeless state; others, after having been subjected to more or less copious bleedings. Thus, then, of the patients of M. de Larroque, only one in ten died; doubtless a very remarkable result, since it would establish a less mortality amongst patients affected with typhoid fever, treated according to M. de Larroque's method, than amongst those treated after other plans. This result had already been published, in an original dissertation, by M. Beau, who has adopted the principles which directed M. de Larroque in the treatment of typhoid fever; and the evidence of M. Beau, a distinguished *interne* of our hospitals, and who, for two years, pursued the researches of M. de Larroque, and accurately observed the patients treated by his *chef*, is not without its importance.

This method, again, has been for a long time the only one employed by many celebrated physicians in the treatment of continued fever, especially when accompanied with the assemblage of symptoms which characterise that form of pyrexia termed bilious. The books of our predecessors are filled with numerous facts which seem to depose in favour of the excellence of this plan; it was adopted by the physicians of Vienna, who had, in this respect, followed the examples set by Stoll, when, in his *Ratio Medendi*, De Haen began to offer great opposition to the employment of emetics and purgatives in fevers, and substituted for them the treatment by diluent drinks. He, however, found many opposers; and Slentz,

speaking on this subject, said, that, to correct one abuse, De Haen had fallen into another.

We may remark that, in a neighbouring country, England, most practitioners treat continued fevers, corresponding in their symptoms to our typhoid fever, by the daily repeated use of emetics, and especially by purgatives, and they are perfectly convinced that this therapeutic method ought to lead the greatest number of these fevers to a successful termination. Finally, one of our honourable countrymen, Dr. Bretonneau, recommended, a few years ago, as the best method of treatment to be pursued in dothinenteritis, the use of saline purgatives, very frequently repeated.

Be this as it may. Since M. de Larroque has announced the result of his researches on the exclusive employment of evacuants in typhoid fever, there are but few physicians of the hospitals of Paris, who, in a more or less sustained manner, have not submitted to this plan some of the patients entrusted to their care; but the greater part, doubtless, feeling that their researches were not sufficiently numerous to be conclusive, have not published them. Dr. Piédagnel was for a long time the only one who had published facts of this kind, observed by him. He treated, by M. de Larroque's method, a hundred and thirty-four cases of typhoid fever; but did not always employ the method in all its purity; thus, sometimes he bled, and very rarely excited vomiting. Of a hundred and thirty-four patients, thus treated by him, he lost nineteen, being a seventh. This is a less satisfactory result than that obtained by M. de Larroque; it is, however, a remarkable one. Besides, M. Piédagnel, in the memoir which he read to the Academy on this subject, has carefully separated the observations relating to slight cases from those relating to serious cases, and he remarked that the evacuant method had much greater effect upon the former than the latter. M. Louis, one of the members of your commission, has, in his wards at La Pitié, subjected to this plan thirty-one individuals *manifestly* labouring under typhoid fever. The results he obtained were published lately in *La Presse Médicale.* Of the thirty-one individuals, twenty-eight were cured, three died; that is, there was nearly one death in ten cases. But among these thirty-one cases, seven were violent, eight moderate, and fourteen slight. Of the three deaths, two belonged to the seven violent cases, and one to the eight moderate. Thus, then, in those cases where danger was already present on commencing the evacuant plan, the mortality was as one in ten.

During the last three years, the reporter of your commission has subjected forty-eight patients to the treatment proposed by M. de Larroque, who had symptoms of typhoid fever sufficiently evident to leave no doubt of the disease. The last patients, thus treated by him, were purposely sent from the *Bureau central* by M. Bouillaud, who, kindly feeling that he could confidently rely on his observation, recommended them to him for the purpose of undergoing this plan of treatment. All those, *without exception*, who, at the commencement of the treatment, were labouring under slight symp-

toms—such as characterise the inflammatory, bilious, and mucous forms—terminated favourably; some speedily, others more protractedly. The number of these was thirty. Eleven other patients underwent the same plan of treatment, who were already in the most dangerous stage, and on whom an unfavourable prognosis might have been pronounced; nine recovered—two were fatal. Seven other patients commenced with M. de Larroque's treatment, when the ataxo-adynamic symptoms had acquired a great degree of intensity, and when the prognosis was very unfavourable; six were fatal. Thus, then, of forty-eight patients eight died, which makes the mortality one in six; consequently a little more than in M. Piédagnel's cases, and greater than in those of M. Louis and of M. de Larroque. If we now compare the four results, viz:—that of your reporter, who had eight deaths in forty-eight cases, making a sixth; that of M. Piédagnel, who had nineteen deaths in a hundred and thirty-four cases, making a seventh; that of M. Louis, who, in thirty-one patients, had three deaths, being a tenth; and, lastly, that of M. de Larroque, who, of a hundred patients, lost ten, making also a tenth;—we find a total of two hundred and thirteen, forty of whom died, leaving the mean proportion of mortality, in the two hundred and thirteen patients, little less than one seventh.

The following, now, are some results furnished by other modes of treatment.

During the years 1833, 1834, 1835, and 1836, the reporter of the commission subjected eighteen individuals affected with typhoid fever, two thirds of which were violent, to the mixed treatment of bleeding and purgatives, (from one to two general blood-lettings; one to three applications of leeches, applying from fifteen to thirty each time; evacuants generally following, sometimes preceding, the blood-letting.) Of these eighteen individuals, six died, elevating the mortality to the considerable proportion of one in three.

During the same years—1833, 1834, 1835, 1836—and the first six months of the year 1832, the reporter of your commission treated twenty-seven individuals labouring under typhoid fever—the symptoms being in two thirds of the cases slight, in the other severe—by moderate bleeding alone, (not exceeding two general blood-lettings, nor more than sixty leeches.) Six of these twenty-seven individuals died, making the mortality a little less than a fourth. The six cases, which proved fatal, were all seriously indisposed at their entrance into the hospital. It is known, and it would be unjust not to refer to it, that M. Bouillaud, by more full and more frequent bleedings, obtained more favourable results.

Lastly, during the same years—1833, 1834, 1835, 1836—and the last six months of the year 1832, the reporter of your commission treated fourteen individuals, slightly affected with typhoid fever, by simple barley water and rigid diet, all of whom were cured.

Thus, gentlemen, of three hundred and seventy-two individuals labouring under typhoid fever, and treated by different methods, but in various proportions, fifty-two died; the mean mortality in the

three hundred and seventy-two being less than one seventh, and, according to the different methods of treatment, as follows:—

Simple diluents, - - - - -	$\frac{0}{0}$ mortality.
Evacuants alone, - - - - -	$\frac{1}{7}$
Moderate bleedings alone, - - -	$\frac{1}{4}$
Bleeding and evacuants, - - -	$\frac{1}{3}$

Shall these results be esteemed conclusive? No, gentlemen, because the facts by which they are produced do not sufficiently correspond either in their number or nature.

Recurring now to the method of M. de Larroque, we may say, as a consequence of the facts above laid down, and without any prejudgment as to the absolute utility of this plan, that several serious cases became unequivocally amended during the daily administration of evacuants; and that the disease, when slight, was not commonly observed to increase under their employment. Indeed, when thus treated, all these cases terminated successfully, according to the results of M. de Larroque, M. Piédagnel, M. Louis, and your reporter. We ought, moreover, to add here that, at one of the sittings of the commission, M. Bricheteau, a member, informed us, without having however counted the cases, that he had arrived at a similar result. If, then, we keep to the facts observed up to this period—to those at least which are known—(and we affirm that they are, in our opinion, far from being sufficiently numerous,) it would appear that we ought to conclude, that, if the evacuant method has not an efficacy sufficiently sustained to prevent the fatal termination of already serious cases when we begin its employment, it at least does not often transform slight into serious cases. It is in fact a very remarkable circumstance, that none of the slight cases, treated thus, have undergone this transformation; yet no doubt would have been entertained of this some years ago, when physicians were under the entire conviction that the administration of an emetic or purgative, in forms of pyrexia corresponding to typhoid fever, must necessarily aggravate them, and promote the supervention of adynamic and ataxic symptoms. We have, however, seen things occur differently under our own inspection; we have observed, during the daily administration of Seidlitz water, the tongue preserve its moistness, or divest itself, without becoming red, of the coats with which it had been covered; the bad taste in the mouth disappear; the thirst rapidly diminish; certain epigastric pains vanish; the frequency of the pulse go on decreasing; the transpiration of the skin abate; the cephalalgia and vertigo lose at once their intensity; the features become expanded; the pains of the limbs, and feeling of lassitude, ameliorate, &c. All these modifications, produced in this manner during the administration of emetics and purgatives, are doubtless neither novel nor surprising to those instructed in the lessons of the masters who preceded us, and in whose works we find them written; but to the greater part of us they appear as facts unknown, and in some degree strange, and hence it seems one of the most

curious studies that we have to pursue; it is entirely an affair of the past, towards which we must recur, submitting it to the test of new experiments. These old doctrines we are called upon to review—not for the purpose of receiving without examination, but to judge them by our more rigorously observed facts, by our more positive modern knowledge, and even by that modern skepticism, which, according to the point on which it turns, may arrest or accelerate the progress of science.

We ought, then, to feel obliged to M. de Larroque for having directed attention to one of the most important questions in therapeutics, and of which, from time to time, so many different solutions have been given. Your commission think that the work of this honourable *confrère* ought to be taken into serious consideration, and that he cannot be too much encouraged to prosecute his researches; but, at the same time, they consider that it would neither be dignified in the Academy, nor for the interest of science, to give a definitive opinion on the value of the treatment which M. de Larroque proposes to employ in all cases of typhoid fever. Before this can be adopted as the best treatment in the generality of cases, it is not only necessary that we shall have collected an hundred or two hundred cases that testify to its efficacy—a much greater number must have been accumulated; we must have prosecuted these observations for several years, at different periods and in different countries, and under the control of those varied atmospheric influences which, by succeeding each other, give rise to such remarkable changes in the severity of diseases, and consequently cause a variation in the number of our successful cases. Who is not aware that in some years all the cases of pneumonia are slight, and are cured, whatever we may do; whilst, in another year, they have all a peculiarly violent character, and are followed by frightful mortality? Who does not know, also, that there is a certain series of months, when, in the surgical wards of our hospitals, all operations are successful; whilst, in a certain other series, they are all fatal? Hence the difficulties—too often insurmountable—which present themselves, when we try to reduce to law a therapeutical result; hence, also, the occasional necessity of being more than once compelled to leave to time—that great *teacher*, as Montaigne said—the decision of such questions; hence, lastly, the objection to invoking the numerical method in their solution,—if we make a premature application of it, do not weigh all the cases at the same time that we count them, and if we do not analyse, scrupulously and minutely, each of the facts to which we are going to give the value of unity.

In order, also, to exhibit the necessity of precaution in the application of statistics to therapeutics, permit me, gentlemen, to cite to you another result, published in London in 1780, by Clarke, in a Collection of Observations on Continued Fevers. From 1777 to 1779, he treated, in the dispensary of that city, two hundred and three individuals affected with continued fever, having all the characters of different forms of our typhoid fever,

violent or slight (the particular cases prove it). Well! of these two hundred and three, Clarke lost only six—that is to say, about one in thirty-three! Success, undoubtedly much greater than that afforded by our modern statistics. Now, how did he treat them? None of these were bled, except two of the three who had inflammatory complications in the lungs. All took, in the first stages of their disease, one or two emetics; they were then submitted to the use of simple diluent drinks, and afterwards all, without exception, took cinchona. If we were only, then, guided by numbers, this would be the best treatment. But this is not a rational manner of proceeding. If even numbers should show to the contrary, we could not persuade ourselves that the numerous varieties of typhoid fever always require the same mode of treatment. We acknowledge, with the majority of the great masters who have preceded us in our difficult and laborious career, that in every disease, whatever be its seat or nature, certain general states of the organism may appear, which may change its aspect, complicate its nature, and modify its treatment. Thus, in any disease, in dothinenteritis, for example, as in every other, the *dynamic* state may deviate from its normal type; and either on account of the constitution or of the idiosyncrasy of the patient, or of the different external influences to which he may have been previously subjected, the forces which govern and co-ordinate the vital functions may be either exalted, diminished in energy, or perverted, and in consequence of this complication of the hypersthenic, hyposthenic, or ataxic state, two diseases, similar in their base, may cease to appear the same, and present differences sufficiently important that, although remaining the same to the anatomist, they may cease to be identical to the therapeutist. In each of them, perfectly special indications may be pointed out to be fulfilled. Thus, there are cases of pneumonia which, in place of treating by blood-lettings, we may be led to combat by cinchona, or by opium. There is no doubt that, in the eyes of Stoll, the bilious form of diseases was singularly exaggerated both in frequency and importance; and yet we can scarcely refuse to admit, as a possible complication of the most opposite diseases, a special state of the organism, to which it has been agreed to give the appellation *bilious*, although its intimate nature may not be known, and which—to the generality of practitioners, ancient as well as modern—has presented particular therapeutic indications. Thus, in the pneumonia before mentioned, in which we referred to the possibility of the employment of cinchona, we may find opportunity for the administration of an emeto-cathartic, and yet in these different cases, to the scalpel of the anatomist, the disease may remain the same; but it is not so to the physician, in whose eyes the anatomical lesion can only be regarded as one of the numerous elements, the knowledge of which may guide him in the adaptation of therapeutic means. Need I again call to mind those different diatheses, idiosyncrasies, special manners of feeling, and reactions, presented in some degree by every new disease you have to treat? If this be the fact—if there be no morbid case bearing an absolute

resemblance to all those that have preceded, and that may follow it —what infinite precautions become necessary, and what errors may be incurred, if, in order to appreciate the efficacy of such or such treatment, we should assume, as the only element of judgment, the number of cases in which it has been employed. We fear, however, in this circumstance, as in many others, that the majority may not always be right, and that, in such a contest, no result is attained if we do not proceed with the strictest spirit of analysis. In fact, we are incessantly operating on quantities which are not of the same nature; if we do not regard this circumstance, must not the most complete deception result? Is this affirming that medical statistics cannot render real service? Far be such a thought from us; but if you would not compromit it, only expect from it what it is capable of accomplishing, and then, when employed with wisdom and discernment, the numerical method will remain—in the possible limits of its application—the most certain method that the physician can employ, in order to distinguish the true from the false, the certain from the uncertain.

As a therapeutic fact, we think we may affirm, that there is not a plan, however unreasonable it may be, which cannot boast of having its statistics; homœopathy and magnetism have had theirs, or can have them; but it must be at once acknowledged, that in such cases the fault is less with the statistics itself, than with those who have made use of it without knowing how. Almost always they have thought, that it was sufficient to collect a greater or less number of facts, and to count them, whilst it was requisite that they should count also the circumstances proper to each of these facts. Now, as these circumstances are infinitely varied, it follows that, in a therapeutic point of view, but very few facts are found which are alike in every condition, and which consequently can be compared; hence the immense difficulty of judging a therapeutical question by cyphers, and hence the necessity—if we wish to have recourse to numbers—for the accumulation of an almost infinity of facts, before endeavouring to deduce from them a result, which may be, therapeutically, the rigorous expression of a truth. When, two ages ago, in the same country, two illustrious physicians, Morton and Sydenham, came forward with a diametrically opposite treatment in small-pox, Morton making use of the tonic and excitant plan, and Sydenham treating it antiphlogistically, neither one nor the other induced, at first, general conviction; in vain might they have published their statistical tables; their numbers would not have decided the question, for conviction is not formed at once; but the mass of practitioners gradually repeated their trials—not a calculable, but an infinite, number of times; by this means, all chances of illusion or error were gradually annulled, and the experience of practitioners has given the verdict to Sydenham; yet they have made their exceptions, and have acknowledged that particular cases present themselves in which Morton's plan may be indicated. Thus therapeutics goes on improving; thus, in every

other department of our science, as in every branch of human knowledge, truth becomes established and sanctioned.

Imbued with these principles, persuaded that the spirit of doubt is the safeguard of science, and the guarantee of its progress, your commission has the honour of proposing to you to render justice to the merit of M. de Larroque, to postpone all definitive judgment as to the efficacy of his treatment, until more amply informed, and until more numerous and more continued experiments have been instituted; to thank M. de Larroque, however, for his interesting communications, and to induce him to continue them, for they will always be well collected; and to deposit his memoir with honour in the archives of the Academy.

REPORT ON ANIMAL MAGNETISM.

MADE TO THE ROYAL ACADEMY OF MEDICINE IN PARIS,

August 8th and 22d, 1837.

M. DUBOIS (D'AMIENS), REPORTER.[1]

As the topic of animal magnetism is at this time engaging the attention, not only of those who are not in the ranks of the profession, but likewise of certain estimable physicians, we think it well to republish the following report, made to the Royal Academy of Medicine in Paris, by some of the best informed members of that learned body,—one of whom, M. Jules Cloquet, is the author of a case in which, during magnetic sleep he is said to have removed a cancerous mamma without producing pain; and another, M. Oudet, is the person who drew the tooth in a case which recently excited great attention in the French metropolis. Yet the report is affirmed to have been agreed on unanimously.

We may remark, that of the reality of certain of the effects ascribed to the manipulations of the animal magnetiser we entertain no doubt. The whole history of the *art* exhibits that highly impressible individuals may have irregularities of nervous distribution induced through the medium of the senses, especially through those of vision and the touch,—and that a kind of hysteric sleep and other phenomena referable to a like condition of the nervous system may be engendered, but that there is any thing like a magnetic fluid or agent that may be communicated from the magnetiser to the subject of his experiments is not only not proved, but by no means presumable.

As to the *clairvoyance* or "lucidity of vision,"—could we assign our belief to it at all, it would be only on the ground,—"*credo quia impossibile est.*"—*R. D.*

The commission met for the first time on the 27th of February, 1837. The rendezvous was appointed at M. Berna's own house. The commission was composed of MM. Bouillaud, Cloquet, Caventou, Cornac, Dubois (of Amiens), Emery, Oudet, Pelletier, and Roux. M. Roux was chosen president, and M. Dubois, secretary and reporter. After a long but amicable discussion as to the course

[1] London Medical Gazette, for Sept. 16, 1837, p. 918, and for Sept. 23, p. 953.

to be adopted; it was agreed by the commissioners and M. Berna that the experiments should take place at M. Roux's house, and that no strangers, except the persons to be the subjects of experiment, were to be admitted into the room.

On the 3d of March the whole commission, except M. Oudet, met at seven in the evening. At a quarter to eight, M. Berna introduced a young girl of seventeen or eighteen, of a constitution apparently nervous and delicate, but with an air sufficiently cool (*dégagé*) and resolute.

The programme of the evening's experiments, which we had sent to M. B., presented eight experiments. The following are their titles, literally copied, for the language does not belong to your commissioners.[1]

1st. Somnambulism.

2d. Proof (constatation) of insensibility to pricking and tickling.

3d. Restitution, by the mental will, of the sensibility.

4th. Obedience to the mental order to lose motion.

5th. Obedience to the mental order, to cease answering in the midst of a conversation; and to the mental order, to answer again.

6th. Repetition of the same experiment, the magnetiser being separated from the somnambulist by a door.

7th. Waking.

8th. According to the mental order which shall be enjoined in the somnambulic state, persistence in the restoration of the sensibility, and also persistence of the power of losing or recovering this sensibility at the will of the magnetiser.

The young girl introduced to your commissioners was received with caution and affability: we conversed with her on indifferent things, and then, to determine, before any attempt at magnetisation, how far she was in her ordinary state sensible to pricking, needles of moderate size, brought by M. Berna himself, were stuck in to the depth of about half a line. Their points were made to penetrate into the hands and neck of this young person, and then, when asked by some of the commissioners, with an air of doubt, if she felt the pricking, she answered positively to M. Roux and M. Caventou, that she felt nothing; her figure, moreover, did not express any pain. Let us remind the academy, that she was at present perfectly and normally awake, by the confession even of her magnetiser, who had not yet commenced any of his manœuvres. This scarcely agreed with the programme, for the insensibility ought not to have been acquired till in the state of somnambulism, or after and by the mental injunction of the magnetiser,—an injunction which could not be given except in this state.

Your commissioners were a little surprised at this singular commencement. What! do you feel nothing? they said to her. But are you absolutely insensible? Then she finished by confessing that she felt a very little pain.

These preliminaries completed, M. Berna made her sit close by

[1] And is therefore not easy of translation.—*Trans.*

him. *Tête à tête* with her, he appeared at first to contemplate her in silence, without practising any of the movements called passes; after a minute or two, he said to your commissioners, that the subject was in somnambulism.

The girl's eyes were covered with cotton and a bandage.

M. Berna had no other proofs to give your commissioners of the pretended state of somnambulism, which, besides, he did not define theoretically, than the experiments comprised in his programme. Then, having again contemplated his somnambule at a very slight distance, he announced that she was struck with general insensibility.

What now could be the part your commissioners should perform? Physicians, surgeons, natural philosophers, all knew that the proofs of the abolition of sensibility are of two kinds; that the one are deduced from the assertions of the subjects—the others from the signs of the external deportment—the language of action. Now the first might be considered as null, when concerned with individuals whose interest is to deceive and lead into error. The mute signs drawn out by pain remained; but then, on the one hand, the intensity of the pain, and on the other the firmness of the patients, had to be taken into consideration. In the present case the intensity of the pain was not to pass certain limits rigorously fixed by M. Berna.

However, some of your commissioners, armed with needles, among others MM. Bouillaud, Emery, and Dubois, set them to prick the poor girl. By word she complained of no pain: her features, as far as we could judge, expressed no painful sensation;—we say as far as we could judge, for her eyes being covered with a large bandage, half her features were concealed from us,—we had scarcely any thing left to observe but the forehead, the mouth, and the chin.

M. Bouillaud, in his trials, did not go beyond the agreed limits; but the reporter, having stuck the point of his needle under the chin with more force, the somnambule made at the moment, and with vivacity, a movement of deglutition. M. Berna perceived it, and gave new cautions.

Touched with the end of the finger by M. Cloquet in the surface of the hand, the somnambule said she felt this impression; so that independently of the perception of temperatures, she had still preserved that of touches,—which, in the system of M. Berna, would add new restrictions to this pretended general loss of sensibility. However, the magnetiser, pursuing the course of his experiments, told the commissioners that he was going, by the sole and tacit intervention of his will, to paralyse, either from sensibility or motion, any part of the girl's body that they would wish. The following conditions were then made:—

That M. Berna should maintain the most perfect silence, and should receive from the hands of the commissioners, papers, on which should be written the parts to be deprived of or endowed with either sensibility or motion; and that he should let them know

by closing one of his eyes that it had been done, and that they might verify it. He said he could not accept these conditions, and gave for reason that the parts pointed out by the commissioners were too limited, and that besides all this was out of his programme, and he did not understand thus the precautions that would be taken against him.

Your commissioners had written—1st, to deprive the chin of sensibility; 2d, the right thumb; 3d, the region of the left deltoid; 4th, that of the right patella. M. Berna had written in his programme, that to show us the sufficiency of his action, he would raise his hand towards us, and that this should be the sign in this experiment as in all the others. This was one of the precautions he had planned; but as your commissioners took good care to look to all these points, they thought they might require of M. Berna, that instead of raising his hand for a signal, he should be content with closing one of his eyes.

As to limits, M. Berna had pointed them out in his programme. For sensibility—1st, the whole of the body; 2d, a part of the body only. For motion, he had written—*a*, the two arms; *b*, the two legs; *c*, an arm and a leg; *d*, a particular arm and leg; *e*, the neck on the right or left side; *f*, the tongue. But here we must explain to the academy what M. Berna understood by paralysis, and by the verification of this paralysis.

All the evidence the commissioners were allowed to have of its existence, was, that when told to raise her arm, &c., the somnambule did raise it or not; in the latter case—that is, if when told to do it, she did not raise her limbs or move her head or talk—she was to be considered as paralysed by the tacit will of M. Berna, and that all this depended on the agency of animal magnetism. Besides this, the commissioners were to make haste with their observations. If the first trials did not succeed, they were to be repeated till paralysis was produced—very good plans for the public, but such as men of science, who were to give an account of their commission, could not exactly comply with. M. Berna then said he would do no more at this meeting, but would wake the somnambule, and at the same time restore her sensibility. M. Bouillaud, at his invitation, was first to place himself behind the girl, ready to prick the back of her neck when the magnetiser gave him the signal. He, M. Berna, placed himself opposite the girl in the same position as the first time. Wake! said he, twice. Then he raised the bandage and the cotton from her eyes, leaned towards her again, put his left arm behind her, and stopped M. Bouillaud, who was of course going to prick her too soon; then leaning towards the girl again, whose eyes were perfectly open, he looks at M. Bouillaud; that commissioner then pricked the somnambule, who turned her head aside, and M. Berna cried out,—There, the sensibility restored! Your commissioners make no reflection on the value of the facts which M. Berna had shown them.

SECOND MEETING.

At half past eight in the evening the same somnambule and all the commissioners being assembled, and the somnambulism having been produced, M. Bouillaud requested in writing that M. Berna would have the goodness to paralyse the right arm only of the girl, and when it was done to indicate it to him by closing his eyes. M. Berna, then sitting near the girl, lowered his head towards her hands, which she held in her lap. The reporter, led by what M. Berna had said, viz., that there should be no contact either immediate or mediate between him and the somnambule, interposed a sheet of paper between his face and her hands.

Presently M. B. made the agreed sign, that his silent will had been sufficiently powerful to paralyse the right arm only of his somnambule. M. Bouillaud proceeded to verify the fact, and for this purpose asked the girl to move successively this or that limb; when he came to the right leg, by way of elimination, as one may say, she answered that she could not move either the right leg or the right arm.

Remember that M. Berna's programme stated that he had the power of paralysing either a single limb, or two limbs at once; we chose a single limb, and there resulted by his own confession, spite of his will, what he called a paralysis of two limbs. The experiment missed, and it was necessary to pass to another; for we had not the politeness, notwithstanding the terms of the programme, to recommence till it succeeded, which certainly must have been soon, since we had only to choose between four limbs and the tongue.

On the 13th of March, at half past seven in the evening, another meeting was held, and the same proceedings were gone through. "Remove from your somnambule," wrote M. Bouillaud on a piece of paper, "remove the power of hearing me, while you stand behind M. Dubois, and then, touching his shoulder, let me know that it is done."

The magnetiser agreed, but wished that the somnambule should be placed very near M. Dubois, who was to act as a screen, and that she should be a foot off him. This was punctually done; the reporter (M. Dubois) made M. Berna go behind him, and hid from him, at least in part, the somnambule, while M. Bouillaud conversed with her in the situation just mentioned; but long before the magnetiser had made the agreed sign, she seemed no longer to hear M. Bouillaud, which showed that the magnetiser's will had acted quicker than he thought; but when the signal was given, then she begins to answer M. Bouillaud, which was precisely the contrary of what ought to have happened.

But as the magnetiser had from the first moment of his transactions with us spoken of these marvellous facts of vision without the assistance of eyes, and of those famous transpositions of the senses, so much talked of in the archives of animal magnetism, you may imagine how desirous we were of seeing such experiments; never had any thing like it been tried before an academic commission.

On the 3d, your commissioners met again, and witnessed the following facts:—At eight in the evening we met at M. Berna's. He was placed by the side of a woman aged about thirty. After our arrival, he covered her eyes with a band, and then told us that she was in a state of somnambulism, and began to talk aloud with her.

Interrogated by her magnetiser (for none of us spoke at this meeting)—interrogated if she saw what was passing around her, this woman declared, that to distinguish objects better, she must turn so as to face him. M. Berna approached her, so that their legs touched, notwithstanding what was said in the programme; but still this was secondary for facts of vision, without the assistance of eyes.

Your commissioners, attentive to what was passing, were, however, penetrated with this idea, that in this sitting there would be two kinds of facts—1st. Those whose solution was proposed to the woman, said to be in somnambulism, but which were known to M. Berna. 2d. Facts whose solution was also proposed to her, but which were unknown to M. Berna, and which would be in part arranged without his knowledge. The latter would have a great value, an absolute value, independent of localities and the morality of the actors, and ought to carry conviction with them. The others would remain subject to various interpretations—to objections more or less founded, and therefore might leave doubt in the mind. Thus, to cite a first instance, the magnetiser commenced by asking the woman how many persons there were present? Several gentlemen, she answered; at least five. This first fact was as well known to M. Berna as to us; and we may add that, approximatively, she herself might know it, since her eyes were not covered till after our arrival.

At the invitation of the magnetiser, who directed every thing in this solemn sitting, the reporter was to write on a card one or several words, that the somnambule might read them. The commissioners, thanks to the officious care of M. Berna, had at their disposal, on a table, two packs of cards, one perfectly plain, the other playing cards. Thus you see the order of the sitting had been obligingly regulated by the magnetiser; there were no more of those hesitations, and those uncertainties, which had, in some measure, disturbed the other sittings; here every thing was arranged beforehand.

However, the reporter wrote on a blank card the word PANTAGRUEL, in printed and perfectly distinct letters; then placing himself behind the somnambule, he presented the card close to her occiput. The magnetiser, seated opposite M. Dubois—that is, in front of the woman—could not see the characters traced on the card; it was a fact of the second order, mentioned above, that is, decisive in itself.

The somnambule, interrogated only by her magnetiser as to what was put behind her head, answered, after some hesitation, that it was something white—something resembling a card—a visit-

ing card. Hitherto, as you may believe, there was nothing to surprise us. M. Berna had said aloud to the reporter to take a card, and write something on it. The somnambule might therefore say she saw something white, like a card; but as soon as she was asked if she could distinguish what there was on this card—"Yes," answered she, firmly, "there is writing on it;" an answer which again did not surprise us. "Is it small or large, this writing?" "Pretty large," she replied. Here, as you see, the serious difficulties commenced, and the somnambule resorted to approximations. "What is written on it?" continued the magnetiser. "Wait, I cannot see plain. Ah! there is first an M—yes, 'tis a word beginning with an M." Such were the first answers of the somnambule.

M. Cornac, unknown to the magnetiser, who alone put questions to the somnambule, then passed a perfectly blank card to M. Dubois, who immediately, and unknown to M. Berna, substituted it for the one which had the word Pantagruel on it. The somnambule still persisted in saying that she saw a word beginning with M. M. Berna, who did not suspect in any way our contrivance, still pressed her with questions; she was invariable; she could only, she said, distinguish a single letter, an M. At last, after some efforts, she added, with some doubt, that she saw two lines of writing.

MM. Oudet and Cornac were then placed behind her; she said she could see one of these gentlemen, M. Cornac. She was asked if he was large? Not very, she said—not so large as you. She was speaking to M. Berna, who alone conversed with her.

M. Cornac, with the consent of the magnetiser, presented in his turn a card to the occiput of the subject, on which he had written the word *Aimé*. She distinguished, she said, some writing, but could not say what it was—what it signified. M. Cornac drew a long purse from his pocket. It is something round, she said; then putting his purse in his pocket again, he presented his hand alone. She said she still saw something round.

After these first attempts, the somnambule complained of being dazzled; that she was annoyed by light. Yes, answered the magnetiser, by fogs; wait—and by means of some fresh passes, he said he had relieved her.

The reporter, charged with taking notes, was writing at this moment within two steps of the somnambule; the point of the pen was heard running along the paper; the somnambule turned aside and raised her head, as if endeavouring to see under the lower edge of her bandage. The magnetiser quickly asked if she saw that gentleman. Yes, she said, he is holding something white and long (the reporter was writing on a paper longer than broad). He then approached the somnambule, placed himself behind her, and ceasing to write, put his pen in his mouth. M. Berna then interrogated his subject in the same manner, that is, on facts of which he had knowledge as well as we. Do you still see, he said, that gentleman behind you? Yes, said she. Do you see his mouth? Not very well.

Why? There is something white and long across it. The magnetiser cast a look of satisfaction towards us, and recommended the reporter to make special note of this fact.

We have taken care not to forget it; but what is its value or importance in relation to the doctrine of animal magnetism. On the one hand, the somnambule knew that she had turned towards some one writing; the distinct noise of the pen on the paper was enough to make this certain, even admitting that she had not seen under her bandage, a trial which she made without opposition on our part; because, as we have already said, we wished to let the magnetiser act without the least appearance of constraint. The reporter still writing, placed himself behind the woman, and then only ceased to write, and put his pen between his teeth. The magnetiser did not take another commissioner for the subject of his questions; but addressed to the somnambule, assuredly without wishing it, a question too indicative—too particular. Do you see that gentleman? Well! but why say—Do you see his mouth? What is there in his mouth? the somnambule might at once ask herself. He has been writing—he has placed himself behind me while writing—can it be his pen that he has put in his mouth?—it is something white and long.

These reflections came at once into our minds, and removed from this fact the value which it might perhaps have had without these circumstances.

In the facts which are about to be presented to you, things could not go on in this manner; varied interpretations were not possible; let us see what was the result.

On a fresh invitation of the magnetiser, M. Dubois wrote in large letters, on a card of the same size as the first, a single word—MISERE—without letting the magnetiser know what it was, and presented it for the somnambule to make out, placed as usual at her occiput. M. Berna's request had been made aloud; the somnambule did not fail to say, without hesitation, that she saw a card, and that there was writing on it. Solicited as before, she seemed to make efforts to distinguish the letters; at last, after great hesitation, she said the word began with a T. The reporter substituted a blank card, and presented it; but neither the somnambule nor the magnetiser could in any way perceive the substitution. Interrogated as to the number of letters, she said she saw five or four. We have said the card was perfectly blank.

Now, gentlemen, we are coming to facts more decisive, more curious, and in which the lucidity of the somnambule was to appear in full evidence. We have already said that M. Berna had prepared, on one of the tables in his apartment, a pack of playing cards. This time, again addressing the reporter, he asked him aloud, and without leaving his intimate relation with the somnambule, to take a playing card, and place it at her occiput. Is it to be a court card? asked the reporter. As you please, answered M. Berna.

This perfectly natural question the reporter had made at first

without reflection, quite innocently; but as he went towards the table on which the pack of playing cards had been previously laid out, the idea struck him not to take either a court or a common card, but while pretending to take a playing card, to take, instead, a perfectly blank one of the same size, still unknown to M. Berna, and we need not add to the somnambule, since she could not perceive substitutions made an inch from her occiput, to which her vision had been transposed.

Then, with his blank card, the reporter placed himself at her occiput, and held it behind her. The magnetiser, seated before her, magnetised with all his force. The somnambule is interrogated—hesitated—made efforts, and said she saw a card; but the magnetiser was not, any more than we, contented with so little. He asked her what she remarked on the card? She hesitated, and then said there was black and red.

The commission let M. Berna continue his manœuvres and his solicitations, that he might clear what still appeared very confused, before the woman's transposed sense, and which as yet consisted only of a little black and red. After some fruitless essays, the magnetiser, undoubtedly but ill satisfied with the functions of the transposed visual sense, invites the reporter to pass his card before the head of the somnambule, close to the band covering her eyes; this was, it may be said, changing the terms of the question, and even of the magnetic doctrine; it was giving up the transposition of the senses, to substitute *clairvoyance* through a bandage. But it mattered little; the reporter passed the cord as the magnetiser wished, but he took care to pass it quickly, and so that M. Berna might suppose he saw only the naturally white back of the card, while the coloured part was turned towards the somnambule's bandage.

The card once in this new position, the magnetiser continued his manœuvres, and solicited the somnambule. She confessed that she saw the card better; then added, hesitating, that she saw a figure. New urging from M. Berna—new solicitations! The somnambule, on her part, seemed making great efforts. After some trials, she declared plainly that she saw a knave! But this was not all; it remained to say what knave, for there are four. Proceeding, without doubt, by way of elimination, she answered her magnetiser that there was black by the side of the knave. Still this was not all; there are two knaves with black at their sides. New urging by the magnetiser—new efforts by the somnambule—new and profound attention by the commissioners. At last she has it. It is the knave of clubs!

M. Berna, thinking the experiment finished, took the card from the reporter's hands, and in presence of all the commissioners, sees and assures himself that it is entirely blank.

As a last operation, leaving both the writing and the playing cards, M. Berna asked M. Cornac for an object he had brought with him, adding, that he would present it in his closed hand before the somnambule's bandage. This object, which we do not mention the name of yet, was given by M. Cornac to M. Berna, and

he, with one hand, presented it close to the somnambule's bandage, and with the other endeavoured to act magnetically on her, and then recommenced the enquiries, solicitations, urgings, &c. She, who had not lost courage, appeared to make great exertions. Her magnetiser asked her if she could distinguish what he had in his hand. Wait! said she. Then, after these feigned or real uncertainties, she said it was something round; then, still pressed with questions, she added, that it was flesh-coloured,—that it was yellow,—and, lastly, that it was of the colour of gold. At new and incessant questions, she added, that it was about as thick as an onion,—that it was yellow on one side, white on the other, and that, lastly, there was black upon it.

Here she complained, and wished, she said, that her magnetiser would finish and wake her: she urgently asked it. Not yet, answered M. Berna,—when you have answered my questions; and then he agitated his hands before her, to drive away obscurities and fogs. Pressed anew to tell the name of the object presented to her, she repeated that it was yellow and white. Do you say it is white? asked M. Berna. (Here the commission incidentally remarked, that M. Berna was perhaps wrong in recalling only the word white: there was in this, as you will presently see, something too indicative—too special.) But the somnambule said positively yellow on one side, white on the other, with black above.

Have you, said M. Berna, such an object? No, said she. Have I? Ah! yes, you have that. But, rejoined he, if you had it, what would you do with it? I would put it on my neck. Solicited, for the last time, to explain herself better,—to say at least the use of the object, if she could not tell the name,—she seemed to collect all her powers, and then uttered only the word *hour;* then, at last, as if suddenly illuminated, she cried out, it was to see the hour. M. Berna returned M. Cornac the mysterious object; it was a silver medal, of the weight and size of a piece worth three francs; on one of its surfaces there was a caduceus, on the other two capital letters.

Thus ended this sitting. Some difficulties have since arisen between the commission and M. Berna, who wished that a copy of the *procès-verbaux* should be given him; and in consequence of the refusal he met with, refused in his turn to proceed to fresh experiments.

HISTORICAL REVIEW.

Some discussions raised in this academy at the beginning of the present year had excited anew the attention of physicians to animal magnetism.

Our colleague, M. Oudet, though not entering into any question of doctrine, had confirmed, at one of our meetings, a fact inserted in some public papers, and afterwards in the *Bulletin de l'Académie*, viz:—that a magnetiser had come on November 14th, 1836, to take him to a young lady who was, he said, in a state of somnam-

bulism; that when he arrived there, the magnetiser had several times, and severely, pricked her; had held her finger for some seconds in the flame of a candle, to try her sensibility; and then, that he, M. Oudet, had unfolded his case of instruments and extracted a large molar tooth; and that, at the instant of extraction, the young lady had drawn back her head a little, and uttered a slight cry. These two signs of pain, he adds, had the rapidity of lightning. Then, after half an hour's sleep, the magnetiser had proceeded to wake the somnambulist, and had apprised her, or at least had said to her, what he had done, to spare her the fright and pain of the operation.

On the 24th of January last, on the question of M. Capuron, these explanations, thus given to the academy, provoked an animated discussion. This made some noise in the medical world, principally, of course, among those then occupied with animal magnetism; and a few days after, viz: on the 12th of February, a young physician, M. Berna, addressed a letter to the academy, in which he undertook to afford those to whom, he said, authority is nothing, personal experience as a means of conviction. The academy thus challenged, took into consideration the perfectly voluntary demand of M. Berna.

Three learned societies had been successively occupied with the question of animal magnetism:—The Ancient Academy of Sciences, in March, 1784. 2d. The Ancient Academy of Medicine, first in August, 1784, and afterwards in the sitting in October in the same year, when Thouret gave an account of the different lectures and memoirs which the society had received from its associates and correspondents on the subject. 3d. The Royal Academy of Medicine, in February, 1826.

It was the authorities who took the first step, in 1784; the king had at first named only physicians, viz: Borie, Sallin, Darcet, and Guillotin, to give him an account of animal magnetism as practised by a M. Deslon; but on the request of these four academicians, the king added to their number, five members of the Royal Academy of Sciences, viz: Franklin, Leroy, Bailly, De Bory, and Lavoisier. Bory dying at the beginning of the undertaking, Majeault, a physician, replaced him.

The magnetiser, Deslon, had engaged, 1st, to prove the existence of animal magnetism; 2d, to communicate his knowledge on this discovery; 3d, to prove its utility in the cure of diseases. Nothing was more easy than to present to the commission a theory, called that of animal magnetism, and certain manœuvres called the practice. This, Deslon did not fail to do; but it was necessary to see the effects. For this the commissioners resolved, first to be magnetised themselves, with this express condition, that no stranger should be admitted where the sittings were held, so that they might freely discuss their observations amongst themselves, and be, in all cases, the sole, or at least the first, judges of what they observed.

The experiments were performed with these conditions, and it was clearly determined that no one of the commissioners had felt

any thing, or at least testified any thing, which was of a nature to be attributed to the action of magnetism.

Several patients were then collected at Passy, at Franklin's, and magnetised in the presence of all the commissioners. These patients belonged to the lowest class of society; others were chosen from higher conditions of life, and then children were magnetised, so as to vary the individual conditions as much as possible. Now, by all these experiments, the commissioners became convinced that the imagination did every thing, and that magnetism was nothing.

The touchings, added the reporter, the imagination, imitation, such are the true causes of the effects attributed to this new agent, known by the name of animal magnetism, to the fluid which is said to circulate in the body, and to be communicated from person to person. Ultimate conclusions:—the magnetic fluid does not exist, animal magnetism is nothing (*nul*), and the means employed to put them in action are dangerous.

This was signed at Paris, on the 11th of August, 1784, by all the commissioners. We shall not speak of either the secret report, or of Jussieu's; the latter was an individual opinion: we have only to consider the reports discussed and adopted by academic majorities. However, the Royal Society of Medicine could not remain stranger to the debates which the question then excited; the government had chosen enlightened commissioners from it, and they had in consequence to draw up a report on the same subject, nearly at the same period. These commissioners were, Poissonier, Caille, Mauduyt, and Andry. Their mode of proceeding was not very different from the above mentioned; the magnetiser was the same; the subjects, patients suffering from various real or imaginary diseases. They concluded that the theory of animal magnetism is a system entirely devoid of proofs; that the means employed to put it in action might become dangerous; and that the treatment by these proceedings might give rise to very serious spasmodic and convulsive accidents. This decision was signed at Paris, August 15, 1784, by all the commissioners.

In answer to various letters which the academy had addressed, they received numerous memoirs from all the provinces of France, from Malta, St. Domingo, Holland, England, Turin, &c., of which Thouret made a *résumé*, from which it appeared that two great and principal reasons had led almost all the physicians in France and abroad to reject and condemn the new practice of animal magnetism. On the one hand, the non-existence of a new agent designated by the name of animal magnetism; and, on the other, the danger of the practices—the manipulations—of the whole system, in short, which was intended to produce the effects attributed to the fluid.

After this the subject was not for a long time revived; the revolution and the long war distracted men's minds from questions of this kind; and it was not till 1825, that a physician, named Foissac, addressed a letter to the Academy of Medicine, demanding whether they did not intend to recommence the examination of animal

magnetism. The academy took the demand into consideration; and on the report of a special commission, commissioners were again appointed to make the enquiry.

Here we shall not enter into the history of all the experiments made in the presence of our colleagues: we respect their convictions, but their report cannot be considered as the general expression of the Academy of Medicine.

Arriving at our own commission, we must first remind you that you had composed it of the representatives of contrary opinions on the question, and of members occupied in various particular scientific pursuits. You sent both classes to the facts, because, on the one hand, whatever were their previous convictions, you had confidence in their good faith; and, on the other, by reason of the variety of their scientific tendencies, you thought they would examine the facts in all their aspects.

Gentlemen, we may at once tell you, this precaution has had, in some degree, its reward. With our various ideas for and against, no difference, as you will see, has arisen among us on the facts of which we have been witnesses; with our varied propensities to consider facts in particular aspects, we have been unanimous in each of our conclusions. You will find, perhaps, in this a new warrant of their truth; for it was necessary that the facts submitted to our examination should have very strong positive or negative evidence, to induce every time a constant unanimity among commissioners always at issue on the theoretic value of animal magnetism. But to proceed to the experiments.

Résumé and Conclusions.

1st. It results, in the first place, from all the facts and all the incidents of which we have been witnesses, that, in the preamble, no special proof was given us of the existence of a particular state called that of magnetic somnambulism; that it was only by assertion, and not by demonstration, that the magnetiser proceeded in this matter—affirming to us at each sitting, and before any experimental trial, that his subjects were in a state of somnambulism.

The programme delivered to us by him stated, it is true, that before the somnambulisation it should be ascertained that the subject of the experiments enjoyed perfect sensibility; that for this purpose we might prick her, and that then she should be put to sleep in presence of the commission. But it results from the trials which we made in the sitting of the 3d of March, and before any magnetic practice, that the subject of the experiments did not appear to feel the prickings before the supposed sleep, any more than she did during it; that her countenance and her answers were nearly the same before and during the so-called magnetic operation. Was this a mistake on her part? Was it natural insensibility, or acquired by habit? Was it to excite unseasonably any interest for her person? Your commissioners cannot decide. It is very true, that on each occasion we were told that the subjects were put to sleep; but we

were told so, and that was all. If, nevertheless, the proofs of the state of somnambulism could result ulteriorly from experiments made on subjects presumed in this state, the value or the nullity of these proofs must be drawn from the conclusions which we shall now draw from these same experiments.

2d. According to the terms of the programme, the second experiment was to consist in the proof of the insensibility of the subjects. But after having mentioned the restrictions imposed on your commission—that the face was put out of sight, and removed from every trial of this kind—that it was the same for all the parts naturally covered, so that there remained only the hands and neck; —after having reminded you that on these parts we were permitted to exercise neither pinching nor scratching, nor the contact of any body, either on fire or of a slightly raised temperature, but were limited to the sticking in of needles to half a line deep;—after recalling all these restrictions, we are justified in deducing from these facts—1st, that none but very slightly painful sensations could be excited; 2d, that these could be produced only on parts perhaps habituated to this kind of impression; 3d, that this kind of impression was always the same, and that it resulted from a kind of *tatonage;* 4th, that the features, and especially the eyes, where painful impressions are more especially indicated, were hidden; 5th, that, in consequence of these circumstances, an *impassibilité*, even complete and absolute, could not be a conclusive proof to us of the abolition of sensation in the subject in question.

3d. The magnetiser was to prove that, by the mere intervention of his will, he had the power of restoring, either totally or partially, sensibility to the somnambule.

But as it had been impossible to prove to us experimentally that he had removed it, and that he had isolated the sensibility in the girl, it was also impossible to prove the restitution of sensibility; and, besides, it results from the facts that we observed, that all the trials made for this purpose completely failed. The somnambule accused every thing but that which had been announced. You know, that for the verification we were restricted to the assertions of the somnambule. Certainly, when she affirmed to the commission that she could not move the left leg (for instance), it was no proof to them that that limb was magnetically paralysed; but then, again, what she said did not agree with the pretensions of the magnetiser: so that, from all this, there resulted assertions without proofs, in opposition to other assertions equally without proofs.

4th. What we have said of the abolition and restitution of sensibility may be completely applied to the pretended abolition and restitution of motion: not the slightest proof was given of it to us.

5th. One of the paragraphs of the programme had for its title—Obedience to the natural order, to cease in the middle of a conversation from answering verbally, or by signs, to a particular person.

The magnetiser endeavoured, in the sitting of the 13th of March, to prove that the tacit power of his will could produce this effect;

but it results from the facts then observed, that, far from producing this result, his somnambule appeared no longer to understand, when he did not wish to prevent her from understanding; and that she seemed to understand anew, when positively he did not wish her to understand; so that, according to the assertions of the somnambule, the faculty of understanding, or not, was completely *in opposition* to the will of the magnetiser. But from these facts, well observed, the commission do not draw any conclusion that there was opposition, any more than submission, to his will; they saw here a natural and complete independence of it, and nothing more.

6th. Transposition of the sight.—Yielding to the wishes of the commission, the magnetiser, as you have seen, left the abolitions and restitutions of sense and motion, to pass to greater facts—that is, to facts of vision without the assistance of eyes. All the incidents relating to these have been stated; they took place in the sitting of April 5th.

By the power of his magnetic manœuvres, M. Berna was to show a woman deciphering words, distinguishing playing-cards, following the hands of a watch, not with the eyes, but with the occiput, which would imply either the transposition or the non-necessity and superfluity of the organ of sight in the magnetic state. The experiments were made—you know how; they completely failed. All that the somnambule knew—all that she could infer from what was said near her—all that she might naturally suppose—she said with her eyes blinded; from which we conclude, in the first place, that she did not want a certain degree of address. Thus the magnetiser asked aloud of one of the commissioners to write a word on a card, and present it at the occiput of the woman; and she said she saw a card, and even writing on this card;—she was asked the number of persons present: as she had seen them enter, she gave approximately the number of persons;—she was asked if she saw the commissioner placed near her, and engaged in writing with a pen whose point made a noise on the paper: she raised her head, tried to see him under the bandage, and said that he was holding something white in his hand; she was asked if she saw the mouth of the same gentleman, who, leaving off writing, had placed himself behind her: and she said that he had something white in his mouth;—from all which we draw this conclusion—that the said somnambule, more exercised and adroit than the first, knew how to make more probable suppositions. But as to real facts to prove the vision by the occiput—absolute, decisive, and peremptory facts—not only were they wanting, and completely wanting, but they were of a nature to give rise to suspicions as to the honesty of this woman, as we shall presently remark.

7th. *Clairvoyance.*—Despairing of proving the transposition of the sense of sight—the nullity and superfluity of the eyes in the magnetic state—the magnetiser wished at least to take refuge in the fact of *clairvoyance*, or vision through opaque bodies.

You know the experiments made on this subject; they present the capital conclusion, that a man placed before a woman cannot

give her the power of seeing through a bandage. But here a more serious reflection presents itself: admitting for a moment the hypothesis (which is very convenient for the magnetisers), that in many circumstances the best somnambules lose their lucidity, and, like common mortals, can no longer see with the occiput or the stomach, or even through a bandage, what are we to conclude of this woman from the minute description which she gave of other objects than those presented to her?—who described a knave of clubs on a perfectly blank card—who, in an academic medal, saw a gold watch, with a white face and black letters—and who, if urged, would perhaps have finished by telling the hour that this watch marked?

If, now, you ask what ultimate and general conclusion we would draw from the whole of the experiments made under our inspection, we should say that M. Berna was without doubt under an illusion, when, on the 12th of February, in this year, he wrote to the Academy of Medicine, stating, &c. Those facts which he promised to show are all known to you; you know, as we do, that they are any thing but conclusive in favour of the doctrine of animal magnetism, and that they can have no relation either with physiology or with therapeutics.

Should we have found any thing else in the more numerous and more varied facts furnished by other magnetisers? This we shall not attempt to decide; but what is very certain is, that if there are still at this time other magnetisers, they have not dared to come out into broad day—they have not dared to accept either academic sanction or reprobation.

(*Signed*) MM. Roux, President,
Bouillaud,
Cloquet,
Emery,
Pelletier,
Caventou,
Cornac,
Oudet,
Dubois (d'Amiens), Reporter.

ON THE

APPLICATION OF THE LIGATURE

TO

ARTERIES OR THEIR TRUNKS

AT A

DISTANCE FROM THE WOUNDED PART, AND NEARER THE HEART.

A Contribution to the Treatment of Traumatic Hemorrhages.

BY CHARLES J. BECK,

Doctor of Medicine and Surgery; Ordinary Public Professor of Surgery, Ophthalmology, and Legal Medicine; Director of Surgical and Ophthalmic Clinics at the High School of Freiburg; and member of various learned societies.

TRANSLATED FROM THE GERMAN,

BY EDWARD G. DAVIS, M. D., OF PHILADELPHIA.[1]

PREFACE.

In this treatise I have endeavoured to determine those conditions of arterial lesion which require the ligature of the artery at a distance from the wound, either on a part of the same branch nearer to the heart, or on the trunk from which it issues.

My views are founded on experience, and have been compared with those of distinguished surgeons. In choosing among my own observations, or those of others, I have held those especially worth communicating, which possess interest for the difficulty of the diagnosis, or the mode of treatment. In these cases, prompt and resolute practice is peculiarly needed, and yet it is precisely here that surgeons often hesitate in forming a conclusion, delay

[1] Ueber die Anwendung der Ligatur an einer von der Wunde entfernten, dem Herzen zugewendeten Stelle der verwundeten Arterie oder des entsprechenden Arterienstammes. Ein Beitrag zur Therapie der traumatischen Blutungen, von Karl Joseph Beck, der Medicin und Chirurgie Doctor, Grossh. Bad. geh. Hofrathe, ordentl. öffentl. Professor der Chirurgie, Augenheilkunde, und gerichtlichen Arzneikunde, Director der chirurgischen und ophthalmologischen Klinik an der hohen Schule in Freiburg, Medicinalreferent des Hofgerichts der Oberrheinschen Provinz, Kreisoberhebartz des Oberrheinkreises, der Kön. Academie der Medicin in Frankreich, der ärzlich naturforschenden Gesellschaften in Freiburg, Heidelberg, Bonn, Würzburg, Erlangen, Leipzig, corresp. od. ordentl. Mitgliede, Ehrenmitgliede der Gesellschaft Badischer Medicinalbeamter zur Beförderung der Staatsarzneikunde und des Apothekervereins im Grossherzogthum Baden. 12mo. pp. 79. Freiburg, 1836.

in striking out any decisive plan of practice, and allow the favourable moment for treatment to pass unimproved. I have often observed the disastrous effect of such delay, especially in medico-legal cases brought within my jurisdiction. May this small treatise, in which I have essayed to settle on a solid basis the best mode of applying the ligature in cases hitherto esteemed doubtful, not prove wholly unserviceable. In this labour I have been guided by the sentiment of Quintilian, "Breve iter per exempla." It is only by constantly following the path which experience points out, that any positive conclusion, or trustworthy result, is to be arrived at.

Freiburg, May 1st, 1836.

Among the hardest problems which art has to solve, ranks, under certain circumstances, the arrest of dangerous hemorrhage. If the bleeding vessel is superficial; if the mechanical disturbance, the separation, which the vessel has suffered, is the cause of the hemorrhage; if there is neither structural nor vital change in the relation between the wounded artery and the surrounding parts, the solution of the problem is easy. On the other hand, the remedial means will be difficult in their application, and uncertain in their result, if the source of the bleeding, on account of its depth, is not recognised with certainty and cannot be exposed, and if this depth, together with the importance of the surrounding parts, render direct operation difficult or impossible. It thus happens, that in cases of hemorrhage, in which nothing short of a prompt, discreet, and decided practice can avert death, the surgeon may find it an extremely embarrassing question, whether to tie the vessel in the wound, or at some point between this and the heart, or trunk by which it is given off, or to apply the ligature to the trunk itself. It is my purpose to lay down some distinct rules under this head, founded partly on my own experience, and in part on that of others.

The general rule obviously is, when an artery, or several arterial branches, have been injured, to apply the ligature to the place of the wound, and, if practicable, to take up both ends of each divided vessel, that nearest to the heart, and that farthest removed from it.

It does not enter into my present plan to consider at length the advantage of the ligature over the torsion. One circumstance is evident at once, that the former process admits of a wider application. Experience has taught me the advantage of tying the vessel, and if the reader will examine the cases reported even by those physicians who are partial to the other mode, he will be led to the same conclusion. From the cases communicated by Blandin,[1] in which hemorrhage was checked by the torsion, and which are adduced as arguments for its favourable operation, it results that the arrest is not always permanent, that the operation is very painful, that collections of pus are formed, that the cellular coat of the

[1] Journ. Hebd. 1835. Nro. 21, p. 1, obs. sur la torsion des artères recueillies dans le service de Blandin par Boyer.

artery suppurates, that the wound proves of bad character, and that sanious discharge follows. Without wishing, therefore, to reject entirely the application of the torsion, I am compelled to consider it as extremely limited, and that the ligature still remains the general practice in important traumatic bleeding.

Magendie[1] prefers the torsion to the ligature, and regards its discovery as one of the most important of modern surgery. He considers, however, only its primary operation, and the physical union which is effected by the elasticity of the external coat, without regarding the subsequent organic process, without which a permanent arrest of the bleeding cannot be effected. Lisfranc[2] is inclined to give torsion the preference. Pertusio's[3] researches do not speak so favourably for it; at least they show the uncertainty of the result, and its rare applicability. By these researches, it is evident that the number of turns to be given to the artery must be proportioned to the calibre of the vessel, and especially to its peculiar organic condition, if the torsion is to have the effect of controlling the bleeding. But can this organic proportion be determined beforehand, and are there not cases in which the elasticity of the artery is altered? An accurate calculation and estimate of this proportion, on which the result thus depends, is often impossible; and, consequently, the twisting of the vessel until it ruptures must lead to an uncertain result.

Hodgson[4] insists that, in every injury of a considerable artery, both ends should be tied, because the anastomoses are so extensive, that by their means a fatal bleeding may occur through the lower orifice. In support of this position, he refers to a case, reported by Lawrence, in which fatal bleeding occurred through the lower end of the brachial artery, from its connections with the small branches of the vessel above. On examination, this end was found open, whereas the upper one was closed effectually by the ligature. That the double ligature is also needed when, in consequence of an injury to an artery with hemorrhage, a swelling forms in the neighbourhood of the vessel, is proved beyond a doubt by a case of Dupuytren.[5] In a pulsating tumour, developed soon after an injury of the radial artery with a penknife, Dupuytren first tied the radial above the swelling. As, however, the tumour soon began again to pulsate, another ligature was applied below, and the result was favourable. In wounds of the arteries, Maunoir[6] recommends tying the vessel above and below, and cutting between the ligatures.

[1] Allg. Repert. d. Med. Chirurg. Journ. d. Ausl. von Behrend, 6 Jahrg. Nro. 11, 1835, Nov. p. 279.

[2] Des divers Méthodes et Differens Procédés pour l'Obliteration des Artères dans le Traitement des Aneurismes. Paris, 1834.

[3] Allg. Med. Zeit., von Pabst. Friedreich's Neuigkeiten aus der prakt. Med. d. Ausl. 12. Hft., 1835, p. 1434.

[4] Traité des Maladies des Artères et des Veins, trad. d. l'Angl. et Augm. d'un grand nombre des notes, par Breschet. Paris, 1819, T. 11, 330.

[5] Leçons Orales de Clinique Chirurgicale. Paris, 1832, T. 1, p. 268.

[6] Mémoires Physiologiques et Pratiques sur l'Aneurisme et la Ligature des Artères. Geneve, 1802, p. 71.

If a wound is broad and open, which is commonly the case in those inflicted with a cutting or felling instrument, if the wound is still recent, if the number of injured branches is not considerable, and the size of the injured vessel sufficiently large, the rule of practice above given is still applicable. The same also holds in penetrating and in contused wounds. But when the injured vessel lies deep, and when the external wound and that of the artery are not in a convenient relation to each other, the application of the ligature, above and below, or even immediately above the injury, is difficult or impossible. In cases of this sort, the wound must be enlarged, or a counter opening made, so as to expose the wound of the artery by the shortest way, which must be previously determined by sounding.[1]

Sometimes, in consequence of the nature of the parts, the depth of the vessel, and the magnitude which it would be necessary to give the incision, neither the enlargement nor the counter opening can be employed to the extent which would be necessary to effect the object; apart from the more remote risks which the performance of so serious an operation would involve. The difficulty of applying the direct ligature depends often on that of determining the precise source of the hemorrhage. The character of the injury, too, may be such as to render the practice of direct ligature dangerous. It is thus in fractures with injury of arteries, in which the incision which lays bare the vessel must also expose the bone; and likewise in parts, the structure and functions of which, in consequence of preceding circumstances, vary materially from their normal state. Direct ligature, therefore, is not always applicable; and, in particular cases, we find ourselves compelled by circumstances, before proceeding to amputation, to attempt tying the injured vessel, or its trunk, above the place of lesion.

Hodgson[2] maintains that tying the artery at any distance from the wounded part, or tying a trunk for injury of a branch, affords no security against the return of the bleeding, which may take place through the lower end, as above mentioned, or even through the upper, by means of anastomosing branches entering the vessel below the ligature. He adduces cases, observed by Earle and Guthrie, in support of this view. In these cases, the ligature should no doubt have been applied to the wounded part, and this was actually done, at a subsequent period, in Earle's case. Admitting, however, that the indirect ligature affords less security afterward than the direct, this furnishes no conclusive argument for giving up the operation. In some cases, this is the only practicable mode of proceeding; in others, it possesses a decided advantage over the direct ligature.

Roche and Sanson[3], in speaking of tying vessels in consequence

[1] Ch. Bell's System of Operative Surgery. London, 1814, vol. ii, p. 413.
[2] Op. Cit., T. 11, p. 333.
[3] Nouveaux Elémens de Pathologie Médico-Chirurgicale, p. Roche et Sanson. Paris, 1833, 3 Ed., 4 vol., p. 115.

of injury, express themselves to the effect, that the direct ligature is harder to apply than the other, because the vessel, lost as it were in the mass of parts, changed in form and situation by the effused blood, is utterly withdrawn from sight, and eludes discovery. The direct ligature excites severe inflammation, for the operation is painful, lacerates the parts, opens a passage for the admission of the air, and gives occasion for large deposites of pus; but it has the advantage that it secures from consecutive bleeding. The indirect ligature, too, has advantages and evils. It is less painful, because applied at a point where the vessel is easily exposed and tied. The consequent inflammation is inconsiderable, because the parts are still sound. On the other hand, consecutive hemorrhage may follow, in consequence of the passage of the blood through the collateral vessels to the wound. Sanson and Begin[1] lay down the ligature of the vessel at the seat of injury as a rule, because the tying of the trunk readily induces consecutive hemorrhage through the anastomosing vessels. Chelius[2] coincides in his view with Hodgson, and holds it necessary in the case of large vessels with numerous anastomosing branches, whether divided partially or wholly, to apply two ligatures, one above and one below, to prevent the hemorrhage, which may occur from the lower end. He remarks, that in wounds of the larger branches of an artery, though this may be tied above the place of injury, consecutive hemorrhage may occur through the anastomosing circuit which so readily forms.

On the application of the ligature to wounded arteries, Bruberger[3] adopts the same view, remarking, that in all wounds, accidental or intentional, the ligature, applied to arrest the hemorrhage, should be placed upon the points of termination, where its action is especially demanded. It is to be remarked, however, that wholly or partially divided vessels, in deep and concealed situations, can by no means always be found and closed in the wound even by the dilatation of the latter to the greatest possible extent. If the bleeding comes from a deep and narrow canal, as in gunshot and penetrating wounds; if the neighbouring parts are destroyed or greatly changed, either by the injury or by the consequent inflammation, effusion, adhesion, swelling, &c.; if they are too important to be injured, and do not admit of an enlargement and examination of the wound; if the bleeding vessel is strongly retracted, and the bleeding does not serve as a guide, because appearing indistinctly at several points and irregularly at long intervals; under these and similar circumstances, it is often difficult or impossible to find the bleeding vessel at the point of injury, and to insulate it sufficiently for the ligature; or this operation when performed may be of no avail. Under these conditions, therefore, we must resort to the

[1] Sabatier de la Médecine Operatoire, par Sanson et Begin, 1822, T. 11. p. 174.

[2] Handbuch der Chirurgie, 1. B. 1. Abth. p. 150. Heidelberg und Leipzig, 1833.

[3] Rust's Handbuch der Chirurgie. Berlin, 1833, 10. B. p. 585.

second mode of applying the ligature, seek out the trunk of the vessel, and tie it in its course. Hodgson, indeed, considers this proceeding insufficient to the end proposed, and calls it an error, to apply to wounds what is true only of aneurisms. In regard to the latter, a mere moderation of, and change of direction in, the current of blood, is sufficient to a cure; but for the former, nothing short of a total permanent interruption will suffice, and this is often rendered impossible by the collateral branches. But admitting, with many distinguished surgeons, that the advantages of the ligature in this and similar cases have been overrated, we must agree with Kreissig, that frequently no other resource remains to the surgeon, and that even an imperfect arrest of the flow, co-operating with the efforts of nature, sometimes proves adequate to the object proposed.

Roche and Sanson[1] give the following rules of practice in injuries of the arteries:—If an external wound occurs, and it can be determined what vessel is injured, and where; and if it be possible to expose and tie it, then must it be tied at the point of injury. When, however, no external wound exists, or when an obstinate external bleeding is present, and the examination does not show what vessel is injured, or shows that the vessel lies deeply concealed in the flesh or between the bones, it is better to tie the trunk higher up. Roche and Sanson maintain that, in such a case, it would be advantageous to place the ligature high up, in order to moderate the influx of blood into the collateral branches.

Lawrence[2] lays down the rule, to tie both the upper and lower end of divided vessels. If only a small external wound is present, this must be widened. If the place of injury of the artery cannot easily be found, if the vessel lies deep, the question then arises whether it shall be tied higher up. The bleeding in this case will generally return, and make it still necessary to apply the ligature at the spot. He endeavours to prove, by facts, how little, in injuries of the arteries, we can trust to tying the trunk above the seat of injury: the anastomotic connection and the consequent collateral circulation is always so great that, after tying the arterial trunk, the bleeding may easily be continued or renewed through the lateral branches.

The views of Samuel Cooper[3] on this subject do not materially differ from those of Lawrence. If an artery of considerable size is partially or wholly divided, the ligature must be applied to both the upper and lower end. A punctured artery must be exposed and likewise tied above and below the opening.

Averill[4] recommends tying an arterial trunk at some distance from the injured place in aneurisms, in injuries of arteries lying so

[1] Op. Cit.

[2] Vorlesungen über Chirurgie. A. D. E. von Behrend. Leipz. 1834, 2 Th. p. 194.

[3] Dictionary of Practical Surgery. London, 1818, p. 530.

[4] Kurze Abhandl. der Operativ-Chirurgie. A. d. E. Weimar, 1829, p. 183.

deep that the vessel cannot be caught in the wound, in the simultaneous injury of several branches of one trunk, and when there is a morbid state of the vascular canal in the neighbourhood of the injured place. The following summary comprehends his principal rules.[1] In traumatic bleeding from arteries, both ends, if possible, must be tied, or the trunk must be compressed; and, when this is not enough, must be obliterated. The source of the bleeding may be obscure or inaccessible. Bleeding from a considerable vessel may occur without external wound, or the latter may be so narrow as to render it uncertain from what vessel the flow takes place. In the first case, the tying of the principal trunk usually suffices. In the second case, if the vessel is of small calibre, the attempt must be first made to check the bleeding by compression of the trunk. If this does not succeed, the ligature must be applied to the wounded part, by tying both ends, or the arterial trunk is tied as nearly as possible to the seat of injury.

Blasius,[2] in describing the operation for aneurism after Antyllus, says that this operation, omitting the first act, is applicable to divided arteries, if the last are accessible at the injured place; for tying the vessel at any distance from the wound does not certainly provide against subsequent bleeding. He also remarks[3] that Hunter's operation for aneurism may be executed in arterial bleedings, when the ligature at the place of bleeding cannot be attempted.

Langenbeck[4] attempts to determine precisely the cases in which an artery should be tied at some distance from the wound, in order to cut off the supply from the injured branches. He lays down the following indications for this mode of proceeding. When the artery is so concealed that it cannot be tied in the wound; in gunshot wounds, when several vessels are torn, and the wound cannot be laid open without injury to important parts; the same holds of penetrating wounds also; in passive bleedings, and in those where the vessels in the wound have so far lost their vitality that the application of the ligature is ineffectual, or are so softened by suppuration, or gangrene, that they would be cut through and torn by tying; when bleeding occurs in wounds, in which the process of granulation has fixed so firmly the end of the vessel that it cannot be again drawn out; when a laceration of several arteries occurs; when, after the application of the ligature, no adhesive inflammation follows, but secondary bleeding occurs from suppuration of the coats of the artery; and lastly, in spurious consecutive circumscribed aneurism. The cases of hemorrhage which, according to Grossheim,[5] require tying above the injured place, or indirect ligature, are the following: *a*, when the artery lies too deep to admit of

[1] Bull. de Therap. T. vii. C. 3. Schmidt's Jahrb. 1835, 7. B. 2. Hft. p. 174.

[2] Handbuch der Akiurgie 1. B. p. 203. Halle, 1830.

[3] Op. Cit. p. 215.

[4] Nosologie und Therapie der Chirurgischen Krankheiten. Göttingen, 1825, 3. B. p. 359.

[5] Lehrb. der Operativen Chirurgie. Berlin, 1830. 1. Thl. p. 119.

applying the ligature to the wound; *b*, in bleedings from osseous arteries, which can neither be checked by styptics nor by the tampon; *c*, in bleedings from enlarged vessels of exuberant growth, and in parenchymatous bleeding; *d*, in secondary bleedings which occur after tying divided arteries, on account of defective adhesive inflammation, or of morbid structure of the arterial coats; *e*, in important bleeding from gangrenous parts.

Delpech[1] proposes the question—In case an accidental injury of a considerable vessel render the ligature necessary, shall it be applied at the place of injury, or at some distance above? He answers this enquiry by giving the preference to the indirect ligature in a recent injury by a stab, or by division, laceration or contusion, when the artery lies deep. If a circumscribed swelling follows injury of an artery, the latter may be exposed and tied at the seat of injury; but, even here, Delpech considers it more effectual to apply the ligature at a distance, and thus avoid the disadvantage of a considerable wound. He makes an exception in favour of the direct ligature only in cases of gaping wounds, where the vessel seems to offer itself, as it were, to the operation. In another passage Delpech[2] expresses himself to the same effect. It cannot but be evident that, while Hodgson goes to an extreme on one side, Delpech is entirely too exclusive in his attachment to the opposite view.

Von Walther[3] acknowledges the necessity of applying the indirect ligature under certain circumstances, remarking that, if the wounded vessel cannot be found in the cavity, the dilatation of the wound is seldom advisable; that the indication then is to expose the vessel or its trunk nearer the heart, and to apply the ligature to the new place. In like manner, Zang[4] expresses himself, and advises, in case of insuperable difficulty or danger in tying the vessel in the wound, to apply the ligature, as in Hunter's mode of treating aneurism. Textor[5] adopts the same view, and remarks, that the ligature, like pressure, may be applied either to the wounded part of a vessel, or at a distance from it, according to the circumstances of the case.

Baroni[6] communicates two cases of ligature of the subclavian and brachial, from which he draws the conclusion, that it is better, in wounds of arteries, to tie the vessel above, than to have to seek both ends in the wound for the same purpose.

From the above statements it appears that both experience and authority point out the indirect ligature as a means of arresting

[1] Chirurgie Clinique de Montpellier. Paris, 1823. T. I. p. 111.
[2] Précis Elémentaire des Maladies Chirurgicales. Paris, 1816. T. V. p. 160.
[3] System der Chirurgie. Berlin, 1833. T. B. p. 216.
[4] Darstellung Blutiger Heilkundiger Operationen. Wien, 1813. T. Thl. p. 206.
[5] Grundzüge zur Lehre der Chirurg. Operationen. Würzburg. 1835. p. 44.
[6] Bulletino Delle Science Mediche, 1835. Diefenbach, Frick, und Oppenheim, Zeitschrift für die Gesammte Medicin. 1836. T. B. 2. Hft. p. 253.

hemorrhage; that, under certain circumstances, this is the only feasible mode; and that, in a few rare cases, it deserves the preference to its rival.

The successful treatment of certain abnormal conditions of the vessels, by the indirect ligature, also countenances its application to bleedings arising from their lesion. Aneurism, in which Breschet[1] expressly discriminates the cases adapted for ligature, and tumours which are produced by morbid dilatation of the arterial branches, are treated with good effect by this method. The cases of Travers, Wardrop, Dupuytren, &c., are so well known that any particular notice of them would be superfluous. It must, however, be conceded that, for the cure of aneurism, it is usually sufficient to diminish the afflux of blood, which is not so much the case in hemorrhage from wounds; and that the treatment of tumours from distension of vessels (angiectasia), in the manner alluded to, does not ensure so fortunate a result as similar treatment obtains in aneurism. The indirect ligature has been applied, not only to tumours by angiectasis, but to those dependent on excessive and anomalous nutrition; but the result has not corresponded to the anticipations which were formed of the practice. Blasius[2] tied the subclavian in one of these cases; Dieffenbach, the crural.[3] In a telangiectatic tumour of the femur, Blasius[4] combined the ligature of the crural artery with partial extirpation, with good effect. Ingles Nicol[5] tied the subclavian above the clavicle in medullary sarcoma of the humerus, which exhibited partial pulsation; and Guthrie,[6] the common iliac in a similar case. In neither instance did the desired result follow. Mott[7] tied both carotids, in order to interrupt immediately the circulation in the parotid, and thereby to check the growth of a frightful tumour of this gland. The patient survived this operation only twenty-four hours. In a scirrhous parotid, Mandt[8] tied the carotid without advantage. Magendie[9] tied the carotid on account of a tumour in the antrum. Apoplexy occurred on the sixth day; the memory had become impaired; the tumour had rather enlarged than diminished.

Lallemand[10] has observed an aneurismal osseous tumour on the upper part of the tibia, extending to the knee-joint; this followed upon a rheumatic swelling, pulsated and felt like a scale. A cure was effected by tying the crural. Breschet compares this with some analogous cases observed by Pearson, Scarpa, and Dupuy-

[1] Mémoires de l'Academie Royale de Médecine. T. iii. ch. 2.
[2] Rust's Magazin, 33. B. p. 540.
[3] Medizinische Zeitung des Vereins für Heilkunde in Preussen. Jahrg. II. Nr. 42, S. 182.
[4] Klinische Zeitsch. f. Chirurgie u. Augenh. Halle, 1836. 1. B. 1. Hft. p. 37.
[5] Gerson und Julius Magazin, 1834, Nov. u. Dec. p. 446.
[6] Lond. Med. and Surg. Journ. 1834, August. Kleinert's Repert. 9, Jahrg. 12, Hft. p. 138.
[7] Froriep's Notizen. Mai, 1835, Nr. 962, p. 256.
[8] Rust's Magazin, 37. B. p. 260.
[9] Journ. Univ. d. Sciences Méd., T. 45, C. 149, p. 125.
[10] Rep. d'Anat. et Chirurg., T. ii. p. 253.

tren, and refers them all to aneurisms. The small branches which penetrate the substance of the bone seem diseased, but the larger vessels of the affected limb always appear uninjured and unchanged. The osseous tissue is affected, and the disease extends from within outward. The cellular portion of the bone is wholly or nearly destroyed; the cavity is enlarged and filled with coagulated blood, which lies in concentric layers, as in aneurism; and the coagula appear to be connected with a branch of an artery. The external table, or the compact substance of the bone, continues, but very much thinner than natural; destroyed at several points—at others, possessing little resistance; in fine, much like a mass of cartilage. The periosteum is thickened. Injection shows that the vessels belonging to the substance of the bone are enlarged, and connect by several apertures with the centre of the aneurismal sac. Breschet remarks, that the ligature is more successful in these than in erectile tumours, because in bones there is but one important vascular branch, or, if several co-exist, their origin is from the same trunk, whereas the soft parts are furnished with numerous twigs connected by complicated anastomosis. On this anatomical ground, nothing can be expected from ligature in fungous tumours of the periosteum.

To the present subject belong ligatures of the superior thyroid, which are recommended and produce benefit in vascular tumours of the part. I have, in a distinct treatise,[1] given some cases in which this ligature was employed, and have shown its advantage in one case of my own. In another instance the operation had little effect on the size of the swelling. It is, however, to be remarked, that in this case hypertrophy seemed to be the first and principal affection, and the vascular development was only secondary.

CASE I.

M., aged twenty-four years, of scrofulous habit, contracted a swelling in front of the neck, which extended over the left side, pushed the larynx toward the right, and in part covered this organ. The tumour had first shown itself eight years previous, but had not attained any considerable size till within two years. The difficulty of breathing, and of swallowing, were in proportion to the size of the tumour; and there were also symptoms of cerebral congestion, such as are usually observed to accompany large wens. The patient came into the surgical ward in the summer of 1833. He had already made use of the remedies commonly recommended for wens, but without experiencing any benefit. The tumour had an elastic character, and the vessels were much enlarged. It was recognised as hypertrophy of the thyroid gland, but the presence of a cyst was admitted as probable, and the vascular character, which was yet regarded only as secondary, was already observable.

[1] Ueber den Kropf. Freiburg, 1833.

I proposed the following plan of treatment. The tumour was to be cut into, in order to empty the cyst, if any; and if none, to check the growth by abundant suppuration. The angiectasia and the hypertrophy were at the same time to be resisted by tying the superior thyroid artery. The operation was performed in accordance with this plan. The parts covering the tumour were divided, and the point of the knife plunged into it. A copious discharge of bright red blood followed. The wound was enlarged; the sound could be moved in all directions, and it appeared extremely probable that a cavity existed. By introducing the little finger, I ascertained that the tumour consisted of a soft, spongy, vascular mass, into which the finger could easily be forced. As the bleeding was severe, a sponge tent was plunged into the wound of the gland, and there secured. Before the bandage was applied, we proceeded to the most important part of the operation—the ligature of the superior thyroid. The wound of the integuments was enlarged above, toward the os hyoides, the artery insulated, and tied before its entrance into the gland, between the latter and the omohyoid muscle. The ligature was attended with unusual difficulty, from the great enlargement of the artery and its consequent deep situation.

Fever occurred on the second day from the operation, and continued twenty-four hours in a moderate degree. On the fifth day bleeding occurred, evidently in consequence of the premature separation of the ligature, which was thrown out by the stream of blood. The bleeding was checked by the tampon; it recurred; the tampon was renewed, and ice applied to the wounded parts. The aqua Binelli had, in the mean time, been applied, but without any advantage. The wound was in part gangrenous. The patient recovered gradually from extreme prostration, and the cure followed after some weeks. The tumour had scarcely diminished in size, but was much less tense, and the operation had evidently, in a functional view, been of service. In this case, therefore, the bleeding was followed by no serious consequence; in others, however, of similar character, it has proved fatal.

In morbid states dependent on sanguineous congestion, the ligature of the corresponding vessel has been attended with better effect than where the augmented vis formativa has developed an increase of the substance of parts. Thus Berton[1] tried ligature of the carotid in epilepsy. The individual had suffered nine years with the complaint—was young and plethoric. The attacks occurred at night. It was surmised that, as all means had failed, and no blow or wound of the head had preceded, an increased flow and diminished reflux of the blood must have occurred during the maintenance of the recumbent posture, and that cure might follow the ligature of the carotid. After the operation the attacks were less frequent, but never ceased—probably because the patient indulged too freely in ardent spirit.

The ligature of an arterial trunk is recommended, when it ap-

[1] Journ. Univ. d. Sc. Med., T. 47, C. 141, p. 351.

pears probable that the branches have been injured in the course of an operation, and when the flow of blood cannot be restrained by compression of the trunk. Graefe[1] recommends tying the carotid before the exarticulation of the under jaw, in case the vessels are enlarged. Mott[2] has tied the carotid in three cases, in removing a portion of the jaw diseased by osteosarcoma, in order to prevent the occurrence of hemorrhage. The tying of the carotid, as a previous measure in removing the under jaw, is now commonly omitted as unnecessary.[3] Goodlad[4] tied the common carotid before proceeding to the removal of a considerable tumour on the face and neck, and thus secured himself against the occurrence of arterial bleeding during the operation.

Many other cases, which will be alluded to in the course of this treatise, go to show the efficacy of the indirect ligature as a means of arresting hemorrhage. The cases in which this mode of treatment must obtain, are—

1. The deep or obscure position of the artery, which is to be regarded as the source of the bleeding, together with the fact that this position is unalterable; here belong, also, those cases of consecutive bleeding, where the enlargement of the wound seems forbidden by the advanced state of the cure, or on any other ground.

2. Such an alteration in the structure and vitality of parts at the seat of injury, that the ligature cannot be applied with safety.

3. When the vessels are destroyed at the place of injury.

4. When a large number of injured arterial branches have a common trunk.

5. The case of the injury of an arterial twig at the point of origin.

6. Bleedings from injured vessels without lesion of the soft parts above, or in fractured bones, where the source of the hemorrhage is concealed.

The deep situation of arteries may render it impossible to tie them at the place of injury. In such cases the enlargement of the wound, in order to lay bare the vessel, cannot always be effected. It is often difficult to know the source of the hemorrhage; nor is this always discovered even by extensive enlargement and numerous incisions. The want of correspondence between the external and internal wounds increases the difficulty. So in the injury of arteries which lie between bones, or in their vicinity. If the artery is laid bare in the attempt to find it by enlarging the wound, a serious complication of the injury will be the consequence. The soft parts are often of such character, that it is impossible to expose the vessel by enlarging the wound, or by a counter opening, with-

[1] Journ. f. Chirurg. u. Augenheilk. 18. B. p. 30.

[2] Langenbeck's neue Bibl. f. Chirurgie und Ophthalmologie, 4. B. 3. Hft. ab init.

[3] Vergleiche M. Jäger über Excisio ossium partialis in Rust's Chirurgie, 6. B. p. 507.

[4] Medico-Chirurg. Transact., 7. V. p. 112.

out injury to important structures. Life, or an important function, might thereby be endangered. When the bleeding proceeds from an osseous artery, and the ordinary means of arrest fail, the indirect ligature must be applied.

I now communicate some cases in which I determined to apply the indirect ligature, as the situation of the wound rendered the other mode inapplicable.

CASE II.

K., twenty-two years of age, and of strong constitution, received a wound in the thigh from a double-edged, very sharp and pointed sword. The injury was inflicted in the winter of 1823. The immediate effect was a profuse hemorrhage, of arterial colour. A tourniquet was placed on the upper third of the thigh, cold fomentation applied to the wounded limb, and the wounded man reached home, half an hour's walk from the place where the accident happened. As soon as he was placed on a bed the tourniquet was removed, but the cold applications continued. The wound filled with a strong coagulum, which projected beyond the level of the surface. As some blood was forcing its way out from time to time—as the swelling of the limb constantly increased—and as the pain, of which the patient, though by no means very sensitive, continually complained, at length became intolerable—I was called to the case. More than four hours had now elapsed since the injury was received. I decided, without hesitation, that a considerable vessel was wounded, and that a primitive diffuse aneurism was present.

As the case had to come under legal cognisance, I entered the proper information, in order to obtain a consultation of the legal surgeons. These coincided with my view of the nature of the case, and suggested calling the privy court-counsellor Ecker, whose advice and assistance were considered desirable. It was concluded to enlarge the wound, which lay on the inner side of the middle of the thigh, in this manner to inform ourselves of the situation of the injured vessel, and if possible to apply the ligature to the point of lesion. Having secured ourselves, therefore, against the return of the hemorrhage, by applying the tourniquet, we enlarged the wound. The finger then gained admission into a deep cavity, extending far up, and filled with coagula. These were removed as far as possible, and the tourniquet raised. The wound immediately filled with blood, but we were unable to determine exactly from what quarter it proceeded. We were satisfied, however, from the depth of the wound, that it was impossible to apply the direct ligature. The result of our consultation was, to apply the ligature to the crural artery, at the upper third of the thigh, so that a sufficient quantity of blood might still be supplied to the injured limb by the profunda. I suggested, in opposition to this plan, that such a proceeding would afford no sufficient security, since possibly the profunda itself was the source of the hemorrhage. I yielded, however, to the unanimous opinion of my older colleagues, who did

not absolutely oppose my views, but thought that the ligature of the crural above the bifurcation might still be effected, in case the operation below this point should be without advantage. It was also thought that the crural, and not the profunda, must be the source of the bleeding—since the tourniquet, applied at the upper third of the thigh, had been able to arrest it. I argued, on the other hand, that the pressure of the instrument in this case acted on the whole circumference of the femur, and accordingly on the branches of the profunda; but that the pressure of the finger merely, if applied over the origin of the pudic and above the bifurcation, would be sufficient to arrest the hemorrhage.

I performed the ligature of the crural by artificial light. The artery was readily found at the upper third of the femur, insulated and tied.[1] The operation seemed to be effectual: the bleeding ceased, although no pressure was made either with the finger or with the tourniquet. Considerable heat remained in the injured extremity, and there was no change in the sensibility. The next day the bleeding again commenced; it was easily checked by pressure on the artery at Poupart's ligament. The next morning I found the patient in a state of great prostration; the leg had attained considerable size; a burning heat was remarked in it; the wound and the whole canal were filled with coagula, and the flow of blood had extended beyond them externally. In this state of things I considered amputation near the trochanter major as preferable to tying the crural artery near Poupart's ligament, as I feared the occurrence of gangrene from the extended effusion and the reduced vital power of the patient. But as the latter measure was now regarded by the medical officers present, among whom were men distinguished by their age and experience, as the appropriate practice, I gave up my objections, and tied the crural artery at Poupart's ligament. In regard to hemorrhage this measure had the desired result; no farther bleeding followed. The next day, however, symptoms of gangrene supervened; in the course of two days the whole extremity had mortified, general sinking occurred, and death soon followed.

In the above case, the ligature should, without doubt, have been at once applied near Poupart's ligament. The experiment of tying the vessel below the division could not prevent the recurrence of hemorrhage, because the injury probably extended to twigs of the profunda. With the repetition of the bleeding, the strength sunk, the supervention of gangrene was favoured by the reduced vitality, and the extravasated mass, which aided the putrefaction, and mechanically resisted the expansion of the small vessels, by which the necessary vital fluid might have been supplied to the parts, and through which the reflux should have occurred. Again, instead of resorting to a second ligature, it would have been more to the purpose to have performed amputation, although this, under circumstances so discouraging, was a doubtful expedient. I communicate

[1] Vergl. Froriep's Chirurg. Anatomie der Ligaturstellen. Weimar, 1830. T. 10. 11. 12.

this case to show, not that the tying of an artery at a distance from the injured place is in itself inadequate to check hemorrhage, but that the choice of a place to apply it is a circumstance of especial importance. The mere tying of the profunda, which, however, was performed by Langenbeck[1] without benefit, was here impracticable, and the application of a compress could have served no purpose.

CASE III.

W., a lively and healthy boy of seven years, brought upon himself an injury of the femur in the winter of 1826, in the following manner. While in school, in the intervals of study, he was playing with a pointed penknife, having a long and sharp blade. The knife slipped out of the boy's hand; to prevent it from falling on the floor, he suddenly brought his legs together, so that the right leg drove the blade of the knife deeply into the substance of the left. So severe a hemorrhage followed, that in a few minutes the clothes were drenched with arterial blood, the seat was inundated, and a swoon followed which threatened instant death. I happened to be near the house, and was present directly after the accident. The wound entered at the inner side of the lower third of the thigh, and crossed the direction which the femoral artery takes in passing through the triceps muscle towards the ham. The ham was tense and somewhat swollen, and, when recovered from the swoon, the patient complained of a painful pressure in this part. The recurrence of the bleeding was prevented by applying the finger to the artery at the source of the pudic; afterwards a tourniquet was placed on the upper third of the femur, and the patient carried into his father's house.

From the quantity of blood which escaped from so small a wound, from its arterial character, from the course of the wound itself, from the full and tense condition of the under part of the thigh, there seemed to me no doubt of the existence of an injury of the femoral artery. I found myself fully sustained in this opinion by several other physicians, and it was resolved to proceed at once to take up the vessel. The enlargement of the wound was judged improper, on account of the depth of the artery at this place, and the extensive injury which must result to the neighbouring parts: it was therefore concluded to tie the femoral artery at the point where the middle and upper third of the femur meet. The insulation of the artery was difficult, as it most generally is in children; a simple round ligature of linen thread was applied, and a piece of linen cloth wet with cold water laid over the wound. There was no bleeding. The ligature came off the thirteenth day, and the cure was complete in three weeks. The extremity to which the accident happened proved no way inferior to the other in nutrition or function.

[1] Bibl. f. Chirurg. 1. B. 1. Hft. p. 242.

Here the favourable result mainly depended on the prompt application of the proper remedy. Had we awaited a return of the hemorrhage, the loss of blood would have impaired the strength, and thus have rendered the operation, when performed, one of doubtful efficacy. Here the ligature of the crural artery below the division could be performed with confidence, because, in this way, we could depend on controlling the source of the hemorrhage. A wound of the crural artery, occurring in a shoemaker, under circumstances resembling those of the above case, was observed by Heister,[1] and cured in like manner. Dupuytren[2] remarks, that the ligature of the artery above the injury has almost always a favourable result, when this is recent, whereas in those of longer standing this does not obtain, because the same disposition to adhesive inflammation in the arterial wound and the surrounding parts no longer continues.

CASE IV.

J. B., 24 years of age, of slender form, but of sound constitution, was injured by the discharge of a gun, on the 21st of February, 1836, at 7 o'clock, p. m. The gun was loaded with large shot, and the distance was about ten paces. He remained where he fell, in a woods, for two hours, in a rather cold atmosphere. Considerable bleeding took place, which gradually subsided under the influence of temperature and of tendency to syncope. The shot entered in the back part of the thigh. There were found, between the nates above, and the ham, 28 wounds. Three others were inflicted in front of the limb by the passage of the shot out, and had a diameter of half an inch. The leg was soon swelled in its whole circumference, and moderate fever was developed on the third day. The patient's strength was greatly sunk. Part of the shot were removed.

The fifth day after the injury, a small fluctuating and pulsating tumour was observed in the front part of the thigh, where the middle meets the lower third, and corresponding in position nearly with the femoral artery. It was attempted, but without success, to prevent the increase of the tumour by pressure. Five days afterward it opened externally, and gave exit to a considerable arterial hemorrhage. The tampon and tourniquet were applied, to prevent the recurrence of the bleeding, and permission was obtained to call in Professor Schwörer to perform the operation, which was plainly perceived to be necessary.

A strongly pulsating tumour, closed by the tampon and by coagula, was now present below the middle third of the femur. The pulsation ceased as soon as the femoral artery was compressed, and returned when the pressure was removed. The boundaries of the

[1] D. ch. de arteriæ cruralis vulnere sanato auct. Reinig in Haller Disp. Chir. Select. Lausannæ, 1750, T. V. 133.

[2] Leçons Orales de Clinique Chirurg. Paris, 1832, p. 295.

tumour were not strongly marked, so that the fact of effusion into the muscular tissue was conclusively proved. On the return of the hemorrhage, the blood pressed out of some of the apertures. The swelling and the bleeding were evidently occasioned by an injury of the femoral artery itself, or of a large branch of this vessel. The form of the tumour was first completed on the fifth day, and the secondary bleeding followed still later, in any considerable degree, as a coagulum had formed from the action of cold and from syncope, and the injury was in the condition of a contused wound. With the commencement of suppuration, effusion of blood took place, then swelling, and finally external hemorrhage. The patient was greatly weakened by the bleeding.

The femoral was exposed and tied above the middle of the thigh. The pulsation of the tumour instantly ceased. The tumour was now opened and the coagulum removed. Although no jet showed itself, yet arterial blood continued to pour into the cavity. No vessel could be found from which the bleeding proceeded. The hollow was moderately filled with charpie. The hemorrhage now ceased entirely and did not again return. The strength was gradually restored, and the wounds healed by suppuration. The functions of the affected extremity are entirely reinstated. The integument mortified on the heel; but the slough separated favourably, and the part readily healed.

The grounds of this mode of treatment, which terminated so happily, were the following. The great weakness and exhaustion of the patient demanded a practice which should avert any considerable bleeding during the operation. This was effected by tying the femoral artery before the tumour was disturbed. The latter was not directly above the artery; the source of the bleeding evidently lay deep, and could hardly have been found and recognised in the tumour. The situation of the tumour, and the ceasing of the pulsation as soon as the femoral was compressed higher up, rendered it exceedingly probable that this vessel, or one of its branches, was the source of the bleeding. The enlargement of the already existing aperture of the wound was made in order to remove the effused blood. Had the cavity been permitted to remain filled with coagula, and these to putrefy from the admission of air, the effect on the neighbouring parts, and, by absorption, on the general system, must have been disastrous. The effused blood would have checked the extension of the collateral vessels, on which the circulation of the limb depended, and in this way have endangered the life of the latter. It was also necessary to provide against the return of the bleeding, in the feeble state of the patient, and this was effected by emptying the tumour; this done, a feeble compression sufficed to arrest the hemorrhage caused by regurgitation into the cavity. In fact it is established by observation, that slight force is usually sufficient for this purpose, after the corresponding arterial trunk has been tied.

I am far from regarding the opening of a tumour, caused by arterial bleeding, as the established practice, since many cases are

successfully managed by indirect ligature, without opening the tumour; but sometimes this course is indicated by particular circumstances, especially when, as in the above case, an aperture already exists, which has only to be enlarged by the aid of an operation.

Von Graefe[1] communicates the following observation. It was found necessary to tie the femoral artery in a case of aneurism consequent on a wound of the anterior tibial. The inflammation of the leg had already advanced to gangrene, the tumour had rapidly increased, and the hemorrhage had repeatedly returned from a small orifice remaining in the neighbourhood of the aneurism. The swelling subsided and the bleeding ceased. At the end of the third week the bleeding returned in quantity from the orifice, and the tumour again formed. It was now slit open, filled with charpie, a circular bandage applied, suppuration induced, and the healing process followed. In such a case the ligature of the popliteal artery, as recommended by Mareschal,[2] that is, in the inferior triangular space of the ham, might be preferable to tying the femoral artery. Apart from the consideration, that the operation is so much the less dangerous the farther from the pelvis it is performed, there is also the advantage that the vessels destined to the joint and to the gastrocnemii are saved. In tying the crural, it is to be feared that the anastomosing vessels may send back the blood into the popliteal, and so into the tumour, as appears to have happened in the above case. A case reported by Smith,[3] also favours this view. The tibial artery was tied a second time in a case of aneurism, on account of secondary hemorrhage. The bleeding recurred, the wound was compressed with sponge, and a cure followed.

A great number of cases in which an artery has been wounded with primary and secondary hemorrhage, or with consecutive aneurism, have been successfully treated by tying the vessel between the wound and the heart.

In an injury of the forearm, Richerand[4] tied the brachial artery on account of the bleeding. The hemorrhage, at first checked, soon returned, but so moderately that it was controlled by pressure. This distinguished surgeon strongly recommends dilating the wound in these cases, and applying a ligature to both orifices. If the bleeding returns, the vessel may be tied at some distance from the place of the primary injury. Ballingall[5] tied the brachial artery in two cases. In one case the radial artery was wounded; the ligature applied above on the same vessel stopped the bleeding, but it returned several times. Tying the brachial permanently controlled it. In another case the ulnar artery was wounded, and the bleeding checked by the same measure.

[1] Journ. f. Chirurgie und Augenheilk. Von Gräfe u. Walther. 3. B. p. 254.
[2] Revue Méd. Sept. 1835. Froriep's Notizen, Nr. 1024. Feb. 1836, p. 187.
[3] Froriep's Notizen, Nr. 946.
[4] Grundrifs der Neuern Wundarzneikunst. Leipzig, 1823, 5. Th. p. 202.
[5] Mag. der Ausl. Lit. d. Ges. Heilk. Von Gerson und Julius, 1828, März und April, p. 494.

In consequence of a penetrating wound on the inner surface of the right forearm, directly over the wrist, and extending toward the ulna, bleeding occurred on the fifth day and was frequently renewed. The bleeding vessel could neither be tied in the wound, nor by making an incision at the point where pressure served to arrest the flow; the tampon was therefore applied. The hemorrhage again returned. The ligature of the brachial artery was then performed by Blasius,[1] at the middle of the humerus: the hemorrhage was checked. Eleven days after, it returned in consequence of the patient hearing some frightful intelligence, but was controlled by pressure continued for a short time, and did not return. The brachial artery was injured in bleeding. In spite of compression, bleeding returned, from time to time. The ninth day a considerable tumour was developed. Dupuytren[2] tied the artery half way up the humerus, and with complete success.

In a case related by Hinterberger,[3] a pretty diffused swelling in the upper arm ensued upon an injury, inflicted by a knife four weeks previous. A ligature applied to the brachial, leaving the wound undisturbed, effected a cure.

Velpeau[4] cites several cases in proof of the advantage of tying the femoral and popliteal arteries, in bleeding from the vessels of the leg, when direct ligature could not conveniently be applied. Boyer[5] tied the femoral, with good effect, in a penetrating wound, in which, to judge from the profuse and repeated hemorrhage, the popliteal or an important branch was injured, fourteen days after the injury. Delpech[6] tied the femoral artery some inches below Poupart's ligament, on account of a diffused and pulsating tumour on the leg, induced by a stab, with such good effect, that no further bleeding followed, and the cure was complete.

A young woman of nineteen was wounded in the leg by a scythe, which cut through the tibialis anticus muscle, and wounded the anterior tibial artery. Profuse bleeding followed. "In order to find the division of the artery," says Martini,[7] "I should have had to cut through the common extensor of the toes, the peroneus longus, perhaps the popliteal, or a part of the gastrocnemius, and still to have searched a long time, for the anterior tibial is not always given off by the popliteal, but often rises from the peroneal artery, and then runs a very different course. Compression was applied. The ninth day after the injury secondary hemorrhage occurred and proved very profuse. The femoral was tied at the middle of the thigh. The hemorrhage was permanently arrested and the case went on well."

[1] Klinisch-Chirurgische Bemerkungen. Halle, 1832, p. 120.
[2] Leçons Orales de Clinique Chirurgicale. Paris, 1832, T. 1. p. 268.
[3] Gräfe und Walther Journ. f. Chirurg. u. Augenheilk. 20. B. p. 252.
[4] Nouveaux Elémens de Médecine Operatoire. Paris, 1832. T. 1. p. 138.
[5] Traité des Maladies Chirurgicales et des Operations qui leur Conviennent. Paris, 1818. T. 2. p. 272.
[6] Clinique Chirurg. T. 1. p. 46.
[7] Rust's Magazin, 16. B. p. 516.

In two cases of false aneurism, supervening upon stabs, Blasius[1] tied the femoral near the bend of the thighs. In both patients the aneurism and the operation wound healed; but one subject died of nervous fever. On examination, the femoral artery was found firmly closed at the point of ligature, between the origins of the epigastric and the profunda; the aneurismal sac was filled with coagulum and well contracted; at its termination was the artery, divided across.

A man of sanguineous temperament and robust constitution, affected with swelling of the thyroid gland, suffered from his youth up with headaches, which he aggravated by the excessive use of spirituous liquors, until at last he became epileptic and insane. In an excess of frenzy he stuck a knife into the upper right side of his neck; profuse bleeding followed, and he fell senseless. Eight or ten minutes after Boileau[2] came and performed the ligature of the carotid on the right side. Three fourths of an hour after the operation was completed, consciousness returned, the headaches and the epilepsy both ceased, and the tumour was dissipated by the continued suppuration of the wound.

Cole[3] tied the common carotid after a gun-shot wound which was followed by hemorrhage. This was at once arrested, but the patient afterwards died, in consequence of the pus having made its way from the larynx to the lungs. In a case in which Langenbeck[4] tied the superior thyroid on account of struma vasculosa, hemorrhage followed, and made it necessary to tie the common carotid; the patient sunk, however, exhausted by loss of blood.

Langenbeck[5] tied a wounded axillary artery with good effect. The same surgeon[6] applied a ligature to the subclavian, on account of a bleeding which occurred in consequence of a stab in the axilla. The blood in this case broke out severely from the wound, but was checked by applying compression for a moment to the subclavian artery above the clavicle. The narrow opening was dilated, in order to tie the axillary artery, but the hemorrhage did not cease. Compression was applied to the subclavian and continued. Langenbeck convinced himself that the axillary was tied, but saw that on remitting the pressure the blood issued from branches which it could not have reached from the axilla, and concluded that either the axillary must be injured again above the place of ligature, or that the bleeding proceeded from one of the circumflexæ humeri, or the subscapularis. He therefore tied the subclavian above the clavicle at the external edge of the scalenus, and no farther hemorrhage occurred.

[1] Klinische Zeischrift für Chirurgie und Augenheilkunde. Halle, 1836. 1. B. 1. Hft. p. 35.
[2] Journ. f. Chirurg. u. Augenheilk. von Gräfe u. Walther. 8. B. p. 646.
[3] Rust's Magazin, 8. B. p. 237.
[4] Neue Bibl. f. Chirurg. u. Ophthalmolog. 4. B. 3. St. p. 579.
[5] Ibid. 3. B. p. 274.
[6] Ibid. 1. B. 3. St. p. 545.

Lallemand[1] has reported a similar case. A tumour had formed in the axilla in consequence of an injury to the axillary artery, and the pulsation of the brachial had ceased. The ligature of the subclavian artery was performed, and the patient recovered at the end of nine weeks. In another case the ligature was followed by mortification of the arm, so that it became necessary to remove it at the joint. The following case, in which the subclavian was tied in consequence of hemorrhage and tumour, connects itself naturally with the above. A negro wench received a wound in the axilla from a broken soup-tureen. Profuse bleeding followed on the fifth day. Afterward, when the wound was nearly healed, the hemorrhage was renewed, and a tumour was found pulsating in the course of the pectoralis major, Segond[2] tied the subclavian above the clavicle, with success.

With equal success Dupuytren[3] performed the same operation, exposing and cutting through the scalenus anticus muscle. In this case a pulsating tumour existed in the axilla, the consequence of a stab. After the operation the tumour disappeared.

In hemorrhage occurring when the healing process is advanced; in secondary hemorrhages following after amputation, at a time when the wound is for the most part closed, and compression is found to be insufficient or impracticable; in such cases, the end is best answered by tying the trunk of the artery above the place of injury. The observations of Zang, Dupuytren, Delpech, and others, favour this practice.

M. Jäger[4] recommends to tie the artery at a greater or less distance from the orifice, and at the place where pressure upon it checks the bleeding. In hemorrhage from an arterial branch, the main artery from which it arises must be found and tied.

Delpech[5] tied the femoral artery at Poupart's ligament, in consequence of hemorrhage, which occurred sixteen days after the amputation of the thigh, and with good effect. In another case, hemorrhage occurred in an amputation wound on the seventh day after the operation. Delpech[6] considered it improper to open the wound in order to apply the direct ligature; he therefore tied the femoral at the upper third of the thigh with good effect; the bleeding ceased immediately, and did not return.

Chelius,[7] appealing to the observations of Zang, Dupuytren, and Delpech, recommends tying the femoral artery below Poupart's ligament, if secondary hemorrhage occurs after the amputation of the thigh, and after the healing of the wound is nearly complete.

[1] Recueil de Mémoires de Médecine, Chirurgie et Pharmacie militaire. Paris, 1834.

[2] Magazin der ausl. Literatur von Gerson und Julius, 1825, Mai und Juni, p. 446.

[3] Magazin d. ausl. Lit. d. ges. Heilk. von Gerson und Julius, Sept. u. Oct. 1827, p. 331.

[4] Handwörterbuch der Gesammten Chirurgie und Augenheilkunde von Walther, Jäger, und Radius. Leipz., 1836, 1. B. 2. Lief. p. 241.

[5] Clinique Chirurg., T. 1. p. 60.

[6] Ibid., T. 1. p. 54.

[7] Heidelberger Klinische Annalen, 3 B. p. 340.

He refers, in this connection, to a case, in which, after the amputation of the thigh, hemorrhage occurred, and the ligature of the femoral was effected with advantage. This surgeon[1] regards the ligature of the trunk of the artery above the wound as especially proper in hemorrhage occurring at a late stage of the healing process.

There is a very interesting case communicated by Wedemeyer,[2] in which, in consequence of osteosarcoma, amputation was effected a hand's-breadth below the hip joint. The fourth week the wound was united, with the exception of some small sinuses. On some sudden exertion hemorrhage occurred, on which Wedemeyer determined to perform the ligature of the femoral artery above the origin of the profunda, precisely at the ligament of Fallopius. The grounds of this conclusion were the following:—1. It was uncertain whether the superficial femoral, or the profunda, were the bleeding vessel. 2. The finding and tying of the bleeding artery in a stump nearly healed, but yet inflamed and suppurating, the parts of which must have already melted into a homogeneous mass, would have required a deep incision, and have proved exceedingly difficult, dangerous, and uncertain. Ligatures in inflamed and suppurating parts are indeed both difficult to apply, and, by cutting through prematurely, cause new hemorrhage (Dupuytren, Boyer, Cruveilhier). 3. By tying at the proposed point, the source of the hemorrhage would be controlled, while the nutrition of the stump would be abundantly provided for by the branches of the ischiatic and obturator arteries. The operation was, in fact, entirely successful.

In applying the indirect ligature, it is before all things important to choose such a place for its application, that the source of the hemorrhage may certainly be controlled. In a case of bleeding from a stump, the source of which was probably the profunda, Guthrie[3] tied the trunk of the femoral, but placed his ligature two inches and a half below Poupart's ligament. The bleeding at first stopped, but returned; amputation was performed at the joint, and the patient died. Dissection, of course, proved that the ligature was applied below the origin of the profunda, which is usually given off from one to two inches below the ligament. In this case, its origin was found at the ligament itself, a circumstance hostile to the success of the operation, because the pulsation of the profunda, lying on the femoral, disturbed the coagulum in the latter, and the consequence was a fatal hemorrhage.[4]

The condition of the artery at the place of injury, and of the neighbouring parts, may determine, whether to place the ligature on a part of the same vessel which is in a sound state, or on the trunk itself. If its vitality is lost, the artery will not be able to

[1] Handb. d. Chirurg., 1. B. 1. Abth. p. 157.

[2] Rust's Magazin, 13. B. p. 45.

[3] On gun-shot wounds of the extremities. London, 1815, p. 113. Wedemeyer, a. a. O. p. 49.

[4] Magazin der Ausl. Literatur d. Gesammt. Heilk. Sept. u. Oct. 1827. p. 358.

carry on those organic formative processes, without which obliteration cannot be effected. By inflammation, suppuration, and gangrene, such changes may be effected in the structure and physical condition of the arterial coats, that the ligature cannot be applied with any confidence in its success. The wound may be of such a nature, as, together with the solution of continuity, to cause considerable disturbance of the cohesion, elasticity, and vitality of the vessel. In regard to the vital condition of arteries leading to inflamed parts, Alison[1] has communicated his own and Dick's observations, at a session of the Anatomical Section of the British Association; and the result appears to be, that the contractile power of arteries so situated, their elasticity, the power they possess during life, and even the peculiar properties evinced for some hours after death, exist in a palpably less degree than in corresponding arteries of sound parts.

Dupuytren[2] remarks that, in primitive wounds, gunshot wounds especially, if the injured vessels do not lie too deep, they can be found, caught, and tied, with sufficient facility. The extensibility of the sound tissues permits us to draw out the vessel, and the cellular tissue, as well as the arterial coats, are still of natural consistence, so that the ligature remains till the fifth, sixth, or seventh day, the coagulum is fully formed, and the result can be depended on. After secondary hemorrhage, the case is very different. The vessels have retracted and shrunken, and are found only with extreme difficulty. Both the vascular and cellular tissues are in a state of inflammation, and have lost their natural extensibility. The ligature easily cuts through, and falls off before the vessel is obliterated. The bleeding often returns from one to two hours after the operation. Nothing then remains but to tie the artery at more or less distance above the place of injury. About eighteen years since, Dupuytren adopted this plan after amputation. Several bleedings successively occurred, and it was impossible to perform the ligature at the wound. Dupuytren tied the femoral, and the patient recovered. In the year 1814, he tied the carotid repeatedly, when branches of this vessel were injured.

It is remarked by Travers,[3] that secondary hemorrhage is induced by ulcerated or gangrenous condition of the end of the vessel. C. Bell[4] observes, that bleeding from an ulcerated surface, proceeding from deep wounds of bad character, or from the cavity of a bone, is to be regarded as of ill omen. If the ligature be applied to parts thus soft, easily lacerated, and partially mortified, the vessel will give way. The bleeding then supervenes on a state of constitution in which the loss of a few ounces of blood may be fatal. It is sometimes in our power to tie the trunk of the artery in cases of secondary bleeding after amputation when com-

[1] Froriep's Notizen, Nr. 927. Januar. 1835, p. 42.
[2] Klinisch-Chirurgische Vorträge übers, von Bech u. Leonhardi. Leipzig, 1834, 1. B. p. 519.
[3] Medico-Chirurgical Transactions. London, 1819, V. iv. p. 453.
[4] System of Operative Surgery. V. ii. p. 386.

pression fails, and the ligature at the wound would be followed by repeated bursting of the artery. In such cases, says Bell, the sound skin must be divided, and the arterial trunk tied.

The ligature between the wound and the heart may, according to Blasius,[1] be rendered necessary by the inflamed state of the wound, by suppuration and change of texture, or by the situation of the vessel. Blasius tied the subclavian below the clavicle in a wound of the axilla. The patient was already exhausted by hemorrhage, and died two hours after the operation. The axillary artery was not injured, and the source of the bleeding could not be determined by dissection.

CASE V.

In the summer of 1825, a young farmer, 20 years old, of very robust constitution, was wounded in the arm by the stab of a penknife, in a fray. So severe a bleeding followed, that he fell to the ground, and remained senseless above an hour, lying in the street and at night. He made his way with great difficulty to a neighbouring shed, where he remained till morning. He then found himself so far invigorated, that he took an hour's walk to ask advice and assistance of a surgeon. The surgeon contented himself with ordering yew-ointment and a simple bandage, without even regulating the diet. When the affair came to the knowledge of the authorities, a systematic treatment was instituted by the legal physicians. The 7th day moderate bleeding occurred; this renewed itself several times; direct compression of the wound was not sufficient to arrest the hemorrhage. A tourniquet introduced into the axilla, and supported upon the shoulder, always checked the bleeding. The tourniquet was loosened from time to time, but the hemorrage returned, and at length reduced the patient to a state of serious exhaustion. It was now judged necessary to adopt more decisive practice, and my assistance was requested.

I found the patient, the 13th day after the accident, much enfeebled; he had been bleeding just before my arrival. The tourniquet was on, and drawn tightly, and the whole limb œdematous. The external wound, above an inch and a half in length, commenced on the external side of the arm, and extended toward the axilla. The sinus was several inches long, and could be traced partly with the finger, perfectly with a female catheter, the end of which touched the axillary artery. A bloody fluid exuded from the wound, which exhibited a flaccid condition, with diminished vitality.

The enlargement of the wound in this case would not have brought into view the injured vessel, which was neither situated near, nor over, the external opening. Moreover, in consequence of the pressure which had been applied, and the means employed for checking the hemorrhage, as also from the reduced state of the vital powers, such a condition of the neighbouring parts was present,

[1] Rust's Magazin, 39. B. p. 387.

that the application of a direct ligature, could it have been effected, was not advisable. I had convinced myself that a very moderate pressure upon the axillary with the finger immediately arrested the hemorrhage, and therefore ventured to regard this as the source of the bleeding, unaided by any anastomotic vessels emptying higher up.

My conclusion, therefore, was to tie the vessel high up in the axilla. The legal physicians present coincided in my views and approved my plan. I exposed the axillary, insulated, and tied it with a single thread. The pulsation of the radial could no longer be felt after the operation. It returned feebly after several days. The wound was simply dressed, a good suppuration followed, the tumour of the arm vanished, and the general state was favourable. The ligature came off the 14th day. At this time a slight bleeding took place from the operation wound, which ceased spontaneously, and did not return. It proceeded in fact from the granulating surface only, and was excited by an interview which the patient had with his mistress. The recovery was perfect. The arm entirely regained its strength, and, when I last saw the patient, was performing its part in severe labour without difficulty.

In the preceding case, the deep situation of the injured vessel, the altered vitality of the arteries at the place of injury, and of the surrounding parts, and the entire control which the pressure of the finger exerted upon the axillary, were the causes which led to the adoption of the indirect ligature. Langenbeck[1] says, that let the axillary artery be wounded where it may, or at whatever point in its course aneurism is developed, there is no resource left but to tie the vessel above the wound or the aneurismal dilatation.

CASE VI.

B. a girl of 18, of strong constitution, and previously in perfect health, had an inflammation of the hand, which she regarded as the effect of frost, and which was accompanied with some gastric symptoms. The inflammation extended to the forearm; some gangrenous spots appeared in the palm of the hand, and sloughed; sanious matter was discharged from the openings, and necrosis of the bones of the metacarpus and forearm followed. This was the condition of things when my assistance was demanded. When I saw the patient, who lived at some distance from me, I ascertained that there was partial exposure and necrosis of the ulna and radius, and that it was necessary to lay open the purulent sinuses and the fascia palmaris. As the patient wished me to visit and attend her daily, she was brought to Freiburg. She bore this little journey very well, and was in a comparatively good condition, when I saw her in the evening after her arrival. In the night a severe bleeding followed, which was checked by the application of cold wet cloths and pressure, while tidings were sent to me. On my arrival, I stopped the hemorrhage by placing my finger on the brachial artery

[1] Neue Bibl. f. Chirurg. u. Ophth. 3. B. p. 272.

at the inner edge of the biceps muscle, above the elbow; this pressure was continued by an assistant. The patient had lost much blood, the source of which I took to be the radial artery, which had come within reach of the ulcerative and destructive process at the wrist. The neighbouring soft parts were elevated by the effused blood into a swelling without definite outline. Whenever the pressure was withdrawn, blood could be seen issuing from the open artery. If the pressure was applied to the radial artery above the wrist, the hemorrhage immediately ceased. I divided the upper angle of the ulcerative opening in the soft parts corresponding to the injured artery, extended the incision two inches, and tied the vessel one inch and a half above the place of lesion. Other incisions were made afterward, on account of purulent collections and necrosis, and pieces of bone were removed. A cure was obtained after the lapse of some months, so complete, that at present all the functions of the injured limb are performed as well as before the accident.

Here it was necessary to apply the ligature at a point above the seat of disease, because at this point the vessel had undergone a softening of its texture, which afforded no security for the success of the operation. The reaction, too, which was requisite for the permanent arrest of the hemorrhage, would have failed to occur in this portion of the artery. Whether the opening in the artery was caused by the contact of a splinter of necrosed bone, or occurred in the way of ulceration and gangrene, could not be determined with certainty. The suddenness with which the hemorrhage came on is in favour of the external injury, to which the softened condition of the vessel would make it especially liable.

This case corresponds very nearly with that related by Browne,[1] in which aneurism followed the wound of a vessel by a sequestrum of bone. Here, too, bleeding occurred suddenly upon a strong movement of the hand. Here, too, the hemorrhage was external, in consequence of a considerable opening of the soft parts already existing. The ligature proved sufficient, and amputation was avoided. Another case occurred to me, in which, from necrosis of the metacarpal bones, the small branch of the radial artery running between the metacarpals of the forefinger and thumb, was injured by a splinter, during a movement of the hand, and considerable bleeding ensued.

The number of cases in which the indirect ligature of arterial wounds may be performed with advantage, on account of inflammation, ulceration, or gangrene of the wounded parts, is not inconsiderable. Graefe[2] communicates the following case:—A farmer, 40 years of age, while mowing, received a prick from his scythe in the ham. The blood, which issued forcibly from the wound, was repressed by a strong bandage. The hemorrhage returned, and from the jet and the direction of the wound, it was concluded that

[1] Schmidt's Jahrbücher der in-und ausländischen ges. Med. Leipz. 1836. Nr. 4. p. 76.

[2] Gräfe u. Walther Journ. f. Chirurg. u. Augenh. 18. B. p. 22.

the popliteal artery was injured. As the patient was much exhausted, it was necessary to secure him against further loss by tying the vessel, and as the inflammatory swelling was of great magnitude, the indirect ligature was preferred, the place chosen being somewhat above the middle of the femur. The case did well.

In a case of gangrene and destruction of the inguinal glands, treated by Gibbs,[1] ulceration of the artery supervened with hemorrhage. At first the femoral artery was tied; but as secondary hemorrhage followed, the ligature of the external iliac was resorted to, and proved successful.

Delpech[2] tied the axillary artery, in a case of gunshot wound, under the following circumstances. The wound directed itself toward the brachial artery. Hospital gangrene supervened and rendered necessary the application of the actual cautery. Eight days after the operation arterial bleeding followed, on account of which the axillary was tied. The bleeding was checked, but six days after broke out from the operation wound. This was occasioned by the dressings applied, which caused ulceration of the artery. The subclavian was now tied below the clavicle. By this the hemorrhage was permanently checked, but the patient died in consequence of the phlegmonous inflammation affecting the injured part. The condition of the wound, in which the gangrene showed itself, determined Delpech, in this instance, not to tie at the place of injury, but above.

In a case[3] of disarticulation of the shoulder, hemorrhage ensued from hospital gangrene, and was attributed to the erosion of the axillary artery. Galtié tied the subclavian above the clavicle, and the bleeding stopped. The patient afterward died of the consequences of the gangrene. It appeared that the axillary was duly tied at the time of amputation, but that the source of the bleeding was a part of the vessel higher up, which was opened by the gangrenous erosion.

Delpech[4] communicates the following case. A gunshot wound on the forearm was followed by gangrene. An incision was made, and the cautery applied. In the progress of the gangrene, hemorrhage appeared from the radial artery. The brachial was now tied so successfully that no farther bleeding appeared. The patient died afterwards with typhous symptoms. Delpech's two cases, therefore, were fatal; but this result occurred from gangrene not from the ligature. The bleeding was arrested by the operation, and thus far the advantage of the practice manifested.

When the artery has been destroyed, either partially or in its whole circumference, at the place of injury, the ligature must be applied at a point nearer to the heart, where the vessel is in a sound state. In aneurism it not unfrequently happens, that in consequence of preceding ulceration or atheromatous disorganisation, of

[1] Froriep's Notizen. 18. B. Nr. 19.
[2] Chirurgie Clinique de Montpellier, T. 1. Paris, 1823. p. 2.
[3] Delpech, Clinique Chirurg. T. 1. p. 17.
[4] Clinique Chirurg. T. 1. p. 20.

constant pressure or long continued distension, that part of the vessel, which formed the seat of injury, is more or less completely obliterated. This circumstance occurs also in false aneurisms, in wounds of arteries with hemorrhage, and even supervenes on a wound of the soft parts covering the vessel. The act of wounding itself, or the consequent ulcerative or gangrenous process, may produce this result; it may even happen by the use of caustics, employed to arrest the hemorrhage. In the case which I next relate, destruction of part of the artery occurred, occasioned apparently by the means applied to restrain the bleeding.

CASE VII.

During the winter of 1834, I was sent for to see a young girl, about 14 years of age, on account of repeated bleeding from an aneurismal tumour, which had supervened on a recent injury. On my arrival I obtained the following account of the case. Thirteen days previous, she had been wounded by a sickle, on the left lower extremity. The wound took an oblique direction, inwards and downwards, on the internal side of the leg toward the ankle, and terminated just below this joint. Profuse bleeding occurred immediately, and was repeatedly renewed. It was attempted to check it by pressure and styptics. On the 9th day, the physician was first informed of the accident, and attended. He found the wound partly healed, and a pulsating tumour at the place closed by a solid projecting coagulum. While he removed this, the bleeding returned, and at length broke out in a jet. A tourniquet was applied, which was tightened when the hemorrhage returned, and loosened as it remitted. It was attempted to check the bleeding with the aqua Binelli, but without success. When I saw the patient, she was greatly enfeebled, pale, and bloodless. The pulse was very small. At the place of injury was a tumour, in which, at the moment of examination, I could detect no pulsation; the tourniquet lay, though loosely, at the upper third of the femur. As the patient was so much reduced, that the return of the bleeding threatened danger to life, I executed, with the concurrence and aid of the health officers (sanitätsbeamten) who were present, the ligature of the tibialis posticus, in the following manner.

The tourniquet was firmly drawn. The tumour was divided in the course of the posticus, and the clot carefully removed with the finger and a wet sponge. I could not detect the vessel at this place, for it evidently was destroyed, partly by the treatment employed, partly by the gradual change of structure. I extended the wound an inch and a half upward in the course of the artery, insulated the latter and tied. I found some difficulty in separating the nerve from the artery in which no trace of pulsation could be detected, although the tourniquet was completely relaxed. On this account I did not immediately put on the ligature, particularly as I resided at a distance from the patient, and therefore wished to be certain of having actually seized the artery. I soon found that the

vessel plainly pulsated above the ligature, and that the wound remained dry.

Ouvrard[1] tied the tibialis posticus above the place of injury in a young man, whose strength was much exhausted by repeated bleeding, and with the best effect, though he did not entirely insulate the vessel. This is not to be justified, at least in regard to the nerves. I insulated the artery to be tied with great care, and hold this very important, although Grillo, in popliteal aneurism, tied vein and nerve with the artery. Del Cole cut the femoral artery, while he endeavoured to open the sheath in an operation for aneurism. He tied the whole bundle, and recovery followed. Grillo has done likewise, in fifteen cases, with good effect.[2] Though from these facts it would appear, that this including of other parts is not so fatal to success as has been supposed; yet must the exclusion of the nerve be regarded as the rule, when it is considered that positive injury must be done by interrupting the flow of nervous influence to the limb. In a case in which the tibialis posticus was wounded high up, Astley Cooper[3] performed the ligature of the femoral. The hemorrhage returned at the end of a week, on which the limb was amputated. Langenbeck[4] mentions a case of injury of the same vessel. The surgeon attempted to check the bleeding by applying a tourniquet to the popliteal. As he gained this point he allowed the instrument to remain on, and gangrene of the limb followed. I have twice observed this effect from the application of the tourniquet. In one instance, a surgeon applied it in a wound of the radial, and allowed it to remain, on which the hand mortified. A surgeon, who wished to secure himself against secondary bleeding after amputation of the thigh, let the tourniquet remain on somewhat tightly drawn, on which mortification of the stump and death ensued.

The ligature of an arterial trunk may be necessary, when several branches are injured, whose great number, deep situation, or smallness, makes direct ligature inapplicable. Pressure in such cases is often insufficient, and its employment impairs the sensibility of the parts, not to mention that inflammation, suppuration, gangrene, nervous accidents, and phlebitis may be induced. I had occasion to observe a case in which tetanus was obviously induced by pressure, employed to restrain hemorrhage: at least no other cause could be assigned. A young and strong man had accidentally stabbed himself in the hand, near the joint. Severe bleeding followed, which was restrained by plugging the wound. The hemorrhage returned several times, and as each time pressure above the wrist on the ulnar artery was found sufficient to arrest it, it was concluded to find the vessel and tie it at this place. The two physicians who attempted the operation, were not able to insulate and

[1] Rust's Magazin, 11. B. p. 157.

[2] Magazin von Gerson und Julius, 1835, Jan. u. Febr. p. 133.

[3] Vorlesungen über die Grundsätze und Ausübung der Chirurgie von Tyrrel. Weimar, 1828. 3. B. p. 160.

[4] Bibl. f. Chirurgie. Göttingen, 1806. 1. B. p. 232.

tie the artery; but the expedient was adopted of applying pressure to the latter, at the place of the artificial wound; on which the bleeding permanently ceased. Tetanus supervened, after some days, and proved fatal. On examination, the ulnar nerve was found inflamed. The spinal marrow, especially its membranes, also bore marks of inflammatory action. Sometimes the ligature of the radial or ulnar becomes necessary from wounds of the arterial twigs in the palm of the hand. The tying of one or both these vessels may also be indicated by vascular distension in this part. In a case of telangiectasia lipomatodes in the palm, the radial was tied by Chelius,[1] on which the appearance of distended vessels diminished, but the adipose tumour (lipom) became more prominent. Wounds on the heel or sole of the foot may require ligature of the tibialis anticus or posticus. In injury of deep branches of the carotid, there is often no other remedy than to tie the carotis communis. I proceed to mention a few cases which I myself treated on the principles already mentioned.

CASE VIII.

In the year 1819 I was requested by the upper officers (oberbeamten) of a district, to visit a young man who, according to the account of the clergyman with whom I had the interview, was in a very dangerous condition, caused by an injury to which, at first, no importance was attached. The young man had, in a fit of ill-humour, induced by liquor, broken a large flask, from which he received a deep incised wound in the palm of the hand, extending toward the joint, injuring the superficial arch and causing copious bleeding. This was checked by a compress and bandage, but again returned. The efforts of two surgeons who employed styptics, both in liquid form and in powder, adhesive substances, pressure, &c., did not avail to prevent the repetition of the hemorrhage.

On the twelfth day after the accident, I saw the patient. The surgeon, who called on and accompanied me, observed that the state of the general health was alarming, as a putrid nervous affection had developed itself, on account of which cinchona and Haller's acid had been ordered. The local symptoms, however, had so far improved, that no hemorrhage had taken place since two days previous. Hardly was this account terminated, when I entered the patient's room, and at the same moment so severe a bleeding occurred that the blood forced its way through the thick bandage and dropped upon the floor.

I made pressure on the brachial artery so as to check the bleeding, and ordered this pressure to be continued. The bandage was removed. The hand was affected with inflammatory œdematous swelling, and the wound had a partially gangrenous character. It was filled with plugs and offensive coagula. I seized the forearm

[1] Medizinische Annalen. Heidelb. 1835, 1. B. 1. Hft. p. 102.

with both hands, so as to compress the radial and the ulnar at the same time, and then allowed the pressure on the brachial to be withdrawn. When I raised the finger from the radial no blood appeared; but when I relaxed the pressure on the ulnar, the hemorrhage immediately commenced. It ceased again when the finger again pressed on the ulnar. Thus the source of the bleeding was manifest.

I exposed the ulnar artery a handsbreadth above the wrist, and tied it. The hemorrhage was permanently checked. The wound was lightly dressed. The prostration of the vital forces had been a consequence of the loss of blood, and as the cause of this was removed, and the supply restored, there was reason to anticipate their speedy restoration. All remedies were omitted, and in a few weeks the health was re-established.

In this case the injury of the superficial arch, which is principally supplied from the ulnar artery, was the cause of the hemorrhage. The experiment with pressure, in fact, proved that the blood was supplied to the wound from this vessel. The number of orifices through which the blood issued, and their small diameter, made a ligature in the wound impracticable. The condition of the wound, which indicated an alteration of tissue and diminished vitality, made necessary the transfer of the ligature to a point at which the parts were supposed to be sound.

CASE IX.

G., a magistrate (beamter), seventy-six years of age, in his early life an officer, suffered from frequent retentions of urine, which increased to such a degree that I punctured the bladder above the pubis. In consequence of this, as the natural mode of evacuating the bladder could not be re-established, he wore constantly an especial contrivance for the purpose. Bent with the weight of years and infirmity, though still of sound mind, he was, one fine evening in the summer of 1824, descending the steps of his house, intending to take a walk. Either from dizziness or inattention—he himself knew not which—he stumbled on one of the lower steps, and, to prevent falling, caught at a post with his left hand. Unfortunately, the owner of the house, a glazier, had a short time before received some plates of glass, which stood exactly where the old man expected to find a support; he consequently grasped the glass, and in so doing received a deep wound in the palm of the hand. As the bleeding was considerable, a surgeon residing in the neighbourhood was at once called. I did not see him till a late hour in the evening. The bandage was so tightly fastened that the hand was greatly swollen, cold, and insensible. I therefore removed it. The bleeding, which now showed itself again, was checked by pressure on the ulnar artery above the wrist, and this was continued until the wound was dressed with compresses and bandaged moderately. An assistant remained with the patient during the night. As no important orifice could be detected in the wound, tying there was not to be attempted.

The next evening the bleeding returned, and was renewed during the night, so that I determined to tie the ulnar some inches above the wrist, after having convinced myself, in the manner mentioned in the eighth case, that this vessel was the source of the hemorrhage. The artery was soon found, insulated, and tied. No hemorrhage ensued, the healing process went on rapidly, and entire recovery followed in a comparatively short time.

In this case also the direct ligature, which I at first essayed to effect, was rendered impossible. A bleeding orifice could not be detected with certainty, although the blood issued copiously from the wound. Every part of the surface, in fact, appeared to furnish its proportion of blood. In this case, recovery could only be expected from tying the ulnar artery. In wounds at the wrist itself, the ulnar or radial may usually be tied with facility at the seat of injury, and I have repeatedly performed this operation with success.

Both the precepts and the practice of the most respectable surgeons favour the mode of proceeding adopted in the last two cases.

Astley Cooper[1] advises, in wounds of the superficial arch, first to tie the ulnar, and then to make trial of compression before proceeding to the ligature of the radial. In those of the deep-seated arch, the radial must be first tied. Roux[2] recommends, in injuries of the plantar arteries, to tie the tibialis posticus; and, in those of the palmar vessels, the ulnar—a course which he himself adopted with advantage. In wounds of the hand, giving rise to copious bleeding, Velpeau[3] prefers tying the ulnar or radial above the seat of injury.

A case treated by Mandt[4] belongs to the present class. A gunshot wound, with injury of the volar arch, was followed by severe bleeding and considerable swelling of the wounded extremity. It was attempted, but in vain, to check the hemorrhage by plugging. The plugs were removed, but the hemorrhage persisted. An incision, four inches in length, was now made through the skin and cellular texture, the ulnar artery found, and ligature applied. The bleeding did not return, and recovery was effected, after a large quantity of sanies had been evacuated.

In bleeding from the palmar arteries, in case of a wound between the metacarpal bones of the fore and ring finger, Dubreuil[5] tied the brachial twenty-one days after the accident, and after compression had been tried in vain. The hemorrhage did not immediately cease, but a moderate pressure sufficed to arrest it, and recovery followed.

Bardili[6] observed the following case. A soldier, twenty-three

[1] Vorlesungen über die Grundsätze und Ausübung der Chirurgie von Tyrrel. 3. B. p. 157.

[2] Nouveaux Elémens de Médecine Operatoire. Paris, 1813. T. 1. P. 1. p. 699.

[3] Ibid. Paris, 1832. T. 1. p. 197.

[4] Rust's Magazin, 22. B. p. 485.

[5] Froriep's Notizen, Nr. 926, Dec. 1831. p. 26.

[6] Würtemberg. mediz. Correspondenzblatt. Kleinert's Repert. 9 Jahrg. 1. Hft. p. 47.

years of age, struck a splinter of glass into the palm of his right hand. To check the bleeding, which was considerable, a surgeon had placed a piece of wood between the bones of the fore-arm, dressed the latter with a circular bandage, and covered the wound with lint, through which, however, the blood constantly trickled; so that at the end of sixteen days, when Bardili was called, the patient was much weakened and very irritable. After applying a tourniquet to the upper arm, the bandage was relaxed, on which the hand appeared œdematous, and the fore-arm exhibited a number of mortified spots. After convincing himself that the blood issued from the radial artery, the surgeon exposed this vessel and tied it. The wound was situated on the inner side of the ball of the right thumb, and extended to the interspace between the metacarpals of the thumb and fore-finger, so that the inner twig of the dorsal branch of the radial artery must probably have been injured. The wound was dressed with ung. digestivum, the gangrenous spots with Goulard's extract. A chill followed on the fifth day, with impeded respiration, and the wound assumed an unfavourable appearance. Improvement followed, however, and at length complete cure.

The above cases show that compression is commonly insufficient, and that it involves consequences which sometimes render its application inadmissible. This view of the subject still obtains, though, in consequence of the improvement of the compressing practice by Graefe, there is less objection to it at present than formerly.

Graefe[1] had occasion to observe considerable hemorrhage in cases of palmar wounds. As the direct ligature of the vessels, in consequence of their anastomoses and numerous orifices, was not very practicable; and the double ligature, as Graefe expresses it—the tying of the radial and ulnar—does not always afford security from secondary bleeding; he recommends a stirrup compress, with which a concentrated pressure on the point of injury can be effected. Struve[2] also observed a case of wound of the deep-seated arch. The hemorrhage was renewed, in spite of an extra compress, which, besides, was not well borne. At last graduated pressure to the hand was resorted to, and the whole limb bandaged in Thedel's method, on which the bleeding was permanently checked and a cure effected.

Other arteries have also been tied on account of hemorrhage, after injuries of single twigs, which, from their situation or number, did not admit of the direct ligature. Abernethy[3] records an injury on the neck, inflicted by a cow's horn. It commenced on the left side of the thyroid gland, and extended to the spine. Branches of both external and internal carotid were injured and torn. Bleeding soon ensued, but was checked by pressure on the common carotid.

[1] Graefe u. Walther Journ. f. Chirurg. u. Augenheilk. 15. B. p. 369. Beschreibung des Compressoriums von Ebel, 17. B. p. 305. T. 4.

[2] Rust's Magazin, 10. B. p. 192.

[3] Surgical Observations on Injuries of the Head and on Miscellaneous Subjects. 3 Ed. London, 1821, p. 117.

The attempts to employ ligature in the wound were ineffectual. The opening was enlarged, and the common carotid exposed. Pressure upon this vessel stopped the bleeding, which returned as soon as the finger was removed. The artery was now tied, and the hemorrhage ceased. The patient died in consequence of the serious secondary injuries, but the operation proved effectual as far as the bleeding was concerned.

Breschet[1] adduces several similar cases, in which the carotid was tied by Dupuytren, Lallemand, and Giroux. In the cases of Dupuytren and Giroux, the hemorrhage did not return, and in this respect the treatment was effectual, although the patients died afterwards from debility. In the case recorded by Lallemand, the bleeding was renewed.

Roux[2] tied the common carotid with the best effect in consecutive bleeding, which took place eight days after an injury received in the mouth from a musket ball. Collier[3] proceeded in like manner with a stab, which passed into the mouth behind the jaw. Bleeding ensued, which could not be controlled by pressure. As the flow of blood into the mouth could not be prevented, and direct operation on the injured parts was impossible, the common carotid was tied, and the patient saved. Herbert Mayo[4] tied the common carotid on account of bleeding from an ulcer of the pharynx, and again in consequence of a wound.

Wardrop[5] tied the right carotid on account of an enormous bleeding from a cancerous ulcer of the palate and tongue. The operation was followed by paralysis of the left side, and two days afterwards by death. In the case of a cancerous sore, similarly situated, occurring at Halle, in a man sixty years of age, in which the hemorrhage proceeded especially from the lingual artery, the ligature of the right carotid was undertaken. The operation was terminated without accident, and the bleeding checked. The third day after, the left side of the body was paralysed, and in two days more the patient died.[6] These two observations, though the issue was unfortunate, argue rather in favour of, than against, the indirect ligature.

When the hemorrhage takes place from the division of an arterial branch at the place of its origin from the trunk, where the direct ligature cannot be applied, and compression does not avail to arrest it, the ligature of the trunk itself becomes necessary. It was in this way that Kluge[7] proceeded, and the following extract from the report of the case, by Rust and Kluge, is to this purpose:—

[1] Traité des Maladiés des Artères et des Veines, par Hodgson t. p. Breschet. Paris, 1829, T. ii. p. 34. 44, 45.

[2] Considerations Cliniques sur les Blessés qui ont été reçus à l'Hospital de la Charité. Paris, 1830, p. 24.

[3] Medico-Chirurgical Transactions. London, 1819, V. 7. p. 107.

[4] Magazin von Gerson und Julius, 1831, July u. August, p. 80.

[5] Langenbeck neue Bibl. f. Chirurg. u. Augenheilk. 4. B. 3. St. p. 595.

[6] Rust's Magazin, 18. B. p. 115.

[7] Rust's Magazin, 14. B. p. 576. Ibid. 15. B. p. 239.

A young man fell from a considerable height upon several beer flasks, so as to receive a wound on the inner side of the leg. The wound bled severely. The sartorius was divided, and the wound reached to Poupart's ligament. The attempt to seize the vessel with forceps was fruitless. Pressure with the thumb on the femoral artery, toward the arch of the pubis, stopped the bleeding. On remitting the pressure, the blood flowed, apparently from the main artery itself. It was conjectured that the source of the bleeding was the epigastric artery, divided just as it issues from the femoral. As any attempt to tie the latter was forbidden, by the circumstance of the orifice existing so near the trunk, nothing remained for the preservation of the man's life, but to expose and tie the main artery above the ligament; on doing this the bleeding ceased. The patient died on the thirteenth day, in consequence of pneumonia, as dissection proved. The examination also showed, that not the epigastric artery, for the knot lay below it, but the external pudic, at its exit from the femoral, was divided. For the rest, the external iliac as far as the ligature, and, directly below it, the femoral, had remained pervious.

The indirect ligature deserves the preference when fractures are complicated with hemorrhage, for the exposure of the fracture would be exceedingly injurious, especially when a condition of things is present, in which it is an object to defend the deep-seated parts from the access of the air, as is the case in comminuted fractures. The effused blood putrefies, and the inflammation rises to a fearful height, or gangrene follows, for the weakened parts do not bear large incisions. The accumulations of blood give occasion to the several inflammations, when the entrance of the air is effected through the openings of the sinus, formed by the effusion, and especially is this the case when the bleeding is not concentrated but diffused.[1] Dupuytren[2] is of opinion that, in those cases where fractures, or gun-shot wounds, are complicated with injury of an artery of the same limb, the ligature of the trunk, at some distance above the wound, is to be preferred to ligature of both ends of the artery, which is often impracticable, and even to amputation, which formerly was regarded as the only resource. He adduces three cases, two of comminuted fracture, and one of gun-shot wound, all with destruction of the bones of the leg, and all connected with arterial injury, in which ligature of the femoral artery was performed with good effect. In fracture with injury of the vessels, Roche and Sanson,[3] following Dupuytren and Delpech, recommend tying the vessel above the place of lesion.

Delpech[4] tied the femoral artery in a comminuted fracture, in which the swelling of the leg, and the pulsation, indicated a considerable injury of the vessel. The tumour disappeared, and entire

[1] Malgaigne Manuel de Médecine Operatoire. Bruxelles, 1834, p. 138.
[2] Klinisch-Chirurgische Vorlesungen, 2. B. 2. Abth. p. 264.
[3] Nouveaux Elémens de Pathologie Medico-Chirurgicale. Paris, 1833. 3 Ed. V. iv. p. 114.
[4] Clinique Chirurg., T. 1. p. 38.

restoration followed. In this case, the exposure of the fractured bone was avoided, which was the more necessary, as the source of the hemorrhage was uncertain.

In the foregoing essay, I have described the conditions under which the indirect ligature becomes necessary. I would here add, that the indirect ligature especially comes in use when loss of blood is to be avoided; for in this operation no danger of hemorrhage is incurred, whereas the dilatation of the wound, in order to apply the direct ligature, may occasion bleeding, notwithstanding the application of the tourniquet. It is for this reason that, in hemorrhage from those vessels which admit of no compression above, the enlargement of the wound and direct ligature are always, as far as possible, avoided. To what extent, however, in such cases, the ligature, at the place of lesion, may be carried, appears from the case related by Gibson.[1] Here the gun-shot wound, which opened the common iliac, was enlarged to the extent of seven inches; the peritoneum divided; the intestines pushed to one side; and then the ligature applied above and below the place of injury. The intestines were injured in two places, and these wounds were closed by ligature. The patient died on the fifteenth day after the injury, in consequence of suppuration of the artery and the peritoneum. The suppuration of the vessel probably occurred from the injury inflicted by the ball, by which its coat was torn, contused, and impaired in its vitality; and, secondly, from the fact, that the blood forced its way into the surrounding cellular texture, and thus separated it from its connections. This case shows that, in direct ligature, secondary hemorrhage may be caused by a condition which in the indirect does not occur. The case reported by Carmichael,[2] which agrees with that of John Bell, shows that the direct ligature, even under unfavourable circumstances, may prove successful. The gluteal artery was wounded by a stab with a penknife. A tumour formed, in which pulsation could be heard though not felt. The tumour was opened, the artery tied, and the patient cured.

In concluding my essay, I have still to remark, that the operation which forms its subject usually produces very prompt effects, and that a change of condition, peculiarly grateful to the humane practitioner, is its almost immediate result. Directly after the operation, the anxiety, which previously had possession of the patient's mind, is changed to encouragement. A simple dressing is now sufficient; the previous pressure is laid aside; the pain, caused partly by the compression itself, partly by the embarrassment of the circulation, is removed; the inflammation diminishes; the suppuration assumes a good character; gangrene is arrested. The state of the strength improves, and by preventing the return of the bleeding, that death-like paleness which always attends upon exhausting depletion, is changed to the hue of health.

[1] Rust's Magazin, 10 B. p. 336.
[2] Magazin von Gerson und Julius, 1834, July und August, p. 88.

ESSAY ON CATARACT.[1]

BY THEODORE MAUNOIR, M. D., OF GENEVA.
MEMBER OF THE MEDICAL SOCIETY FOR OBSERVATION AT PARIS.

TRANSLATED FROM THE FRENCH BY HENRY I. BOWDITCH, M. D.
Fellow of the Massachusetts Medical Society, Member of the Society for Medical Observation at Paris, &c. &c.

If, in the study of cataract, we should limit ourselves to the determination of its seat, its nature, its principal symptoms, and chief varieties, with the surgical means employed in its cure, nothing could be better known than this affection is at present. In fact there is perhaps not a single disease whose history is more simple, plain, and uniform, and in which the effect of treatment is more manifestly efficacious.

But if we enter into detail, and ask the precise value of these assertions made by different authors, the proofs which have served as their bases; if we seek for an answer to certain questions of which these dogmatical works do not even speak; especially if we wish to know the truth when there are opposite opinions held by men of the highest merit,—for example, of the comparative value of the two great modes of operating, upon which, however, so many long and sterile discussions have been written,—we soon become convinced that authors have given upon this, as well as upon very many other subjects, rather the vague impressions of their experience, than the results of a rigorous analysis of accurately observed facts.

It is to this imperfect method to which alone the aphorism, *experientia fallax*, applies, that we must refer the contradictory opinions which we meet at every step in medicine, and the total neglect of many subjects which it would be interesting and often useful to be acquainted with. If it were otherwise, if the constant change of facts were the cause of the variety of opinions upon the same subject, which we meet with among the most eminent men, not only in regard to the history of diseases, but especially in reference to methods of cure, we might give up all hopes of ever having any science in medicine. In fact, in order that a science may exist (I speak of a science of observation) it is not sufficient merely to collect even an infinite number of the most circumstantial and exact facts; we must find their relations to one another, and the limits

[1] Mémoires de la Société Médicale d'Observation, tom. i. art. ii. Paris, 1837.

of their variations. Now how can we find these relations if they do not exist, or the limits of their variations if these variations are infinite?

Happily it is not so, and, as I have already stated, the custom which many authors have of appealing to their memory only, and to the authority of others, or to some peculiar exceptional cases, many of which are frequently inexact, explains perfectly the differences of opinion which exist, as well as the omissions and even evident errors which occur. Is there, in fact, any memory so powerful that it can retain in *all* their detail *all* the facts which present themselves to observation, and can afterwards examine their elements, giving to each one only its real value; in a word, make a complete enumeration?

Whatever simplicity, under many points of view, the history of cataract may present, as I have already mentioned, it is sufficient to compare the results of observation with what has been written and with what authors have failed to speak of when treating of the subject, in order to be satisfied that there are many deficiencies to be supplied, some errors to be rectified, and almost every where positive values to be given for approximations. In other words, all that can be said has not yet been written upon this affection, and probably is not likely to be so very soon. This was the opinion of Dr. Tartra when he terminated his thesis for the *concours*[1] in the following manner:—"Nothing could be more important, in order to perfect the pathological and therapeutic doctrines of cataract, than the collection of a very great number of well-authenticated facts; the comparing of them together with reference to their different causes; the consideration of them under the point of view of their acknowledged incurability; the different operations when applied to cataracts supposed to be curable; finally, the more or less perfect cures and other results not less various which may be obtained."

I would certainly not pretend to remedy, in the least, the imperfections which I have mentioned, but merely as I had occasion to collect rather a large number of histories of cataract, I am about to state what I have seen without meaning to deduce any other conclusions than those strictly contained within the limits of my facts.

During the whole of the year 1830, the autumn of 1832, and the spring of 1833, I was constantly in attendance at the surgical visits of La Charité, and excepting the first six or seven operations performed in the spring of 1830, in reference to which I have no details at all, I made records of all the cases indiscriminately which

[1] I dislike to leave any word untranslated, and yet I know of nothing in the English language which will express the signification of "*concours*." In France, when an office in a scientific institution becomes vacant, a *concours* or public exhibition is held, and candidates present their claims by writing and publicly defending dissertations, &c.—*H. I. B.*

presented themselves for operation during these four seasons. My notes unfortunately are not complete in relation to all the points; that is to say, they do not answer all the questions I could ask; but if they are incomplete, they are not inexact; if many are wanting in the details which it would have been very useful for me to have known, they at least do not contain any thing which is not the faithful expression of what I observed with all the care of which I was capable.

These explanations were necessary in order for the reader to understand why the results of individual questions rest upon different numbers, for I could of course never make use of observations except they were complete in the point of view with which I might be occupied.

Finally, being determined to know positively the influence of certain accidents which sometimes cause a complication of the operation itself, and which the uncommon skill of Professor Roux cannot always prevent, I never failed to place myself near enough to the patient to see as exactly as possible the delicate manœuvres which in this operation, more than in any others, are of the highest importance.

The observations I have collected are one hundred and twenty-one in number,[1] which makes the mean number of patients for each of the four seasons about thirty; for it is the custom of M. Roux to operate on cataract, at least, at La Charité only in the spring and autumn.

I shall therefore give in the following order the different particulars in the history of this disease of which I am to treat:—

1. Causes.
2. Origin, course of the disease.
3. Symptoms and diagnosis.
4. Length of time elapsing between the commencement and perfect formation of the cataract.
5. Description of the operation, its difficulties and accidents.
6. Consequences of the operation.
7. Results.
8. Prognosis.

[1] It is fortunate for observation, that the treatment of diseases of the eye, especially of cataract, has ceased to belong exclusively to certain individuals, who for a long time monopolised this branch of the healing art. These affections may now be studied in all the Paris hospitals, and the fact that all surgeons operate for cataract cannot injure any branch of our science.

ARTICLE I.

CAUSES.

The effects of sex, age, constitution, profession, state of health, suppression of the menstrual or hemorrhoidal flux, external violence or accident, and of the hereditary nature of the disease, must be passed in review.

I. *Sex.*—Out of one hundred and twenty-one patients affected with cataract who were received into La Charité, there were sixty-one males and sixty females.[1] This number is sufficiently large, doubtless, for us to infer that if cataract attacks more frequently one of the sexes than the other, the difference cannot be very great. I am well aware, however, that a difference has been mentioned by some writers; it has been said that men are more subject to it than women. Thus, for example, in the "Journal Complementaire des Sciences Médicales," vol. 41, there is a note by Professor Fabini, according to which, out of five hundred persons affected with cataract who presented themselves to his notice, two hundred and sixty-eight were males and two hundred and thirty-two females, making nearly fifty-three males and forty-seven females in one hundred; which, as may be seen, is a very slight difference. We find likewise, in vol. 3 of the "General Repertory of Pathological Anatomy and Physiology," a table of the operations performed at Hôtel-Dieu by Professor Dupuytren from 1815 to 1821. This table contains two hundred and seven patients, one hundred and thirty-five of whom were males, seventy-two only females. This result, which is apparently so very different from those I have just given, is however the confirmation of them, since the number of beds for males in Hôtel-Dieu is very near double the number of those destined for females.

II. *Age.*—It is well known that cataract is a disease of old age. There is, perhaps, not one affection which is more peculiar to this period of life; but, as if to confirm the general rule, that there is nothing absolute in the phenomena of life, we find examples of this disease at all ages. It is even not extremely uncommon to see congenital cataract, for one surgeon, Saunders, operated upon sixty of this kind during the last three years of his too short career.

Nevertheless, we must admit that the number of congenital cataracts, or of those which are formed in early youth, is very small, compared with those which come on at an advanced period of life.

Out of a hundred and twenty-one patients at La Charité,[2] not one

[1] The reader will remember that the number of beds occupied by the surgical patients at La Charité is very nearly equally divided between males and females.

[2] For a long time, the administration was opposed to the admission of children into La Charité. Some may suppose this to be one reason why no patient, of all those whose histories I have examined, was less than twenty years old; but this can be scarcely possible. In fact, Professor Roux tells

was less than twenty years old; and I am certain that it is very rarely that we meet, at the Children's Hospital, with any case of blindness caused by opacity of the crystalline lens or of its capsule; but we frequently meet with it in consequence of perforation of the eye, or opacity of the cornea, in consequence of ophthalmia.

A hundred and twelve patients, affected with cataract, may be separated according to ages as follows:—

From	20	to 29	years	5	patients.
	30	" 39	"	3	"
	40	" 49	"	11	"
	50	" 59	"	25	"
	60	" 69	"	41	"
	70	" 82	"	27	"

On the other hand, in the note by Professor Fabini, to which I have referred, the five hundred subjects affected with cataract are thus classed:—

From	1	to 10	years	14	patients.
	11	" 20	"	16	"
	21	" 30	"	18	"
	31	" 40	"	18	"
	41	" 50	"	51	"
	51	" 60	"	102	"
	61	" 70	"	172	"
	above	70	"	109	"

These two tables demonstrate the immense influence which age has upon the production of cataract, since the frequency of this affection augments in a very considerable ratio as we approach old age; that is to say, an age at which the number of individuals alive is evidently very much less than at an earlier age. Thus it is manifest, that the twenty-seven patients who were between seventy and eighty-two years of age, represent a much larger proportion than the twenty-five who were between fifty and fifty-nine years old.

I must remark, that it is not merely in a general manner, and within very large limits, that nature confines herself to the laws which she seems to have imposed upon herself, but that these laws hold good when we examine the details of the question. Thus, in both of these tables, one made at Paris, the other in Hungary, we perceive that, between fifty and sixty years of age, there is found a quarter or a fifth of all the patients affected with cataract; about a third between sixty and seventy; a little less than a quarter above seventy.

me that it is *very rare* that he is obliged to operate for cataract upon very young subjects. I went daily for months to the Children's Hospital without even meeting a case; and M. Rufz, who, during two years, discharged the duties of clinical pupil (interne), in this place, met with only one case of a child affected with cataract.

It is, then, absolutely true, that at this age cataract is very rare, which, however, was known before.

It may be urged as, in some measure, an objection, that the age of the patients at which they are operated upon, is not that at which they begin to have cataract, which last is, in fact, the only circumstance of importance to be known, since a thousand circumstances may cause these individuals to remain blind many years. With regard to the epoch at which cataract commences, the following are the results of sixty-four cases:—

Cataract began from	20 to 29	years of age, in	4	patients.
	30 " 39	"	5	"
	40 " 49	"	11	"
	50 " 59	"	19	"
	60 " 69	"	21	"
	70 " 79	"	4	"

There is not a very great difference, we perceive, between this table and the two previously given; the number of patients increases and decreases at the same period of life.

I ought to add, that males and females are found almost as frequently in each of the periods of ten years.

III. *Professions.*—Certain trades have been thought to cause a predisposition to cataract; as, for instance, those in which the eye is exposed to the action of very great heat, as the blacksmith's, locksmith's, glassblower's, cook's, &c. &c.; those in which the eye is exercised in examining very minute and brilliant objects, or which make it necessary for the eye to be frequently in contact with irritating vapours; finally, those in which persons are obliged to remain whole days exposed to the full light of the sun. It was for this reason that Petit, of Lyons, calculated that three quarters of the individuals suffering from cataract were cultivators of the soil.

I examined in relation to the employment of a hundred and eight—fifty-four males, fifty-four females. Out of the fifty-four males, thirty, in fact, had been very much exposed, in consequence of their condition, to changes of temperature, and heat of the sun: twelve had been soldiers for a long time, some of whom had been in all the campaigns of the empire—the other eighteen had been farmers, labourers in the country, drivers of the public coaches: there were two blacksmiths; one glassblower, who had worked near the furnace during ten years, his cataracts not having commenced until eight years after leaving this trade; and one jeweller, who, during four years, had been employed in fusing the metals. The remaining twenty-four had been engaged in more sedentary pursuits, or in those in which they had not been exposed to such influences as the others had been; they had been porters, domestics, pedlars, &c. &c.

Not a single patient presented himself who had been obliged to fix his eyes upon very minute and brilliant objects, or had been exposed to irritating vapours.

With respect to the females, twelve were employed in country

labour; six at needle-work; five were washerwomen; the remainder were domestics, porters, housemaids, &c.; in fact, they had not professions which exposed them to the influences which are said to produce cataract.[1]

IV. *Constitution.*—The form of the patient, with the general appearance of his features, which mark the temperament, were noticed in sixty-four subjects—thirty-five men, and twenty-nine women.

Among the men, thirty were of a strong constitution, and their complexion hale; they were, in general, muscular, some of them were even somewhat athletic, though advanced in years. The majority were moderately fleshy; some of them considerably so. Some, on the contrary, were quite thin; in five, the constitution was rather weak than robust.

The colour of the hair was noticed in only twenty-five men. It was—or rather it had been—black, or of a chestnut hue, in eighteen; light or red in seven. I think I can remember that almost all the men were bald, even those who were not very old; but as I have not mentioned the fact in my notes, and as I trust but little to vague recollections, I do not give the fact as certain.

Of the twenty-nine women, seventeen had, at the time of examination or previously, black or chestnut hair; their complexion was more or less hale; in six, the hair was light-coloured; seven were quite fleshy; five, on the contrary, were thin. The others presented nothing very remarkable in this respect.

One of the features in the constitution which it is interesting to know, is the colour of the eyes. It was noticed in sixty-four subjects. The iris was of a more or less deep brown colour, in twenty-four; blue, but more frequently bluish-gray, in twenty-one; ash-coloured in two. In seventeen others, the iris had a greenish tint, and was of a very deep green colour in some, but more frequently there was a grayish, or even a bluish hue, as chief shade, mixed with a well-marked reflection of green. These different colours were very nearly equally divided among women and men. The former, however, presented more frequently than men, examples of brown eyes.

It has been said that short-sighted persons are rarely affected with cataract. Out of forty-nine individuals, whom I conversed with in reference to the condition of their eyesight before cataract began, forty assured me that it was very good, and that they were likewise very far-sighted; in nine only was it short or bad.

V. *Health.*—Out of ninety-three persons, about whose habitual state of health careful enquiries were made, eighty-seven had, in general, been in good health; the greater number never remembered to have had the slightest indisposition. Of the last six, some

[1] I forbear, at present, drawing any conclusions relative to the influence exerted by professions upon the production of cataract. It will be the same with respect to the effect of constitution, and the different features which characterise it, &c. &c. I shall at some future time mention my reason.

were old men, probably affected with emphysema, and they had been troubled with shortness of breath and with cough for many years. Two complained of having had, at times, trouble in the digestive functions. Another had severe accesses of coughing for many weeks previous to entering the hospital. But no one of them considered himself ill.

An oculist, M. Lusardi, has made the same remark. "*I have verified*," says he, in his memoir upon cataract, "*that the greater number of those affected with this disease, whether male or female, enjoyed good health.*" Hence we must conclude that cataract is generally independent of every other species of morbid affection of the system; which is, by no means, a fact of frequent occurrence in other diseases.

VI. *Headache.*—This symptom has been thought to be of very great importance in the history of cataract. Let us see whether the coincidence is frequent.

Sixty-three patients were examined for this purpose; eight men out of thirty-six had been more or less subject to pains in the head, but in two only did these seem to have any connection with the origin of the cataracts. One was an old man aged eighty-two years, and he experienced some very slight pains during several nights at the commencement of the cataracts. The second, aged fifty-five years, had been liable from earliest childhood to violent attacks of hemicrania, which returned three or four times a month, occupying sometimes the right, at others the left, side of the head, and frequently obliging the patient to go to bed during an attack. This hemicrania had finally diminished spontaneously, and then ceased altogether. It was at this period (four years before my examination) that one of his eyes began to lose its power; otherwise his health was excellent.

The six remaining patients said merely that they had been more or less subject to pains in the head, but they had either ceased many years before the commencement of cataract, or had not changed at that time, or, finally, they had not appeared until after the vision had become already somewhat obscured.

In the twenty-seven women, pains in the head were more frequent. Sixteen of them, in fact, without regarding themselves as in any other way ill, had been frequently subject to headache. In eight only did it seem to have any relation to the cataracts. Thus, five of them had not been subject to it before their eyesight began to lessen; but at this period, or at a short time previously, more or less severe pains in the head began and continued a considerable time. In one of the women, a pain in the forehead began with the cataracts, continued nearly uninterruptedly during a year, was sometimes very violent, and then ceased spontaneously. In another, pains began at the same time that the vision began to be obscure, but they occurred only occasionally, lasting two or three days each time. In a third, they were very painful and lancinating, commencing at the back of the head and shooting forward to the forehead; they continued during a few minutes and then ceased, but

usually reappeared two or three times during the day, and scarcely ever remained more than two days without being felt, &c. &c. The three other women had been for a long time subject to pains in the head, when their powers of sight began to lessen, but at this period there was a great increase of them. Finally, in the last eight there seemed to be no more relation between the headache and the cataracts than there was in the cases of the six men of whom I have last spoken.

VII. *Ophthalmia.*—Since some authors consider capsular cataract as rather frequent, and as inflammations of the external part of the eye might have some influence upon the more deeply seated portions, it becomes interesting to examine whether habitual and long continued ophthalmias coincide in a great number of cases with cataracts. Fifty-eight patients were examined upon this point and seven only had had chronic ophthalmias of a severe character, generally consequent upon small-pox. In three of these seven patients there remained very slight opacities upon the cornea, but no traces of inflammation. In three more the eyelids continued to be the seat of chronic inflammation; the conjunctiva was red, and the lids partly destroyed. There was likewise in three of the patients, at the commencement of their cataracts, a little pain and redness of the eye during three or four weeks in one, and during two and five months in the two others. Finally, in two patients who previously had never experienced any ophthalmias, the eyes had been somewhat red for some weeks at the epoch at which they were operated upon.

VIII. *Suppression of Hemorrhoidal or Menstrual Discharge.* There is scarcely any disease in the production of which the suppression of some habitual discharge, whether natural or otherwise, has not been made to play a great part, and especially have writers spoken of the effect of hemorrhoidal and menstrual discharges in this respect. It is true that such opinions have not been held with regard to cataracts particularly; nevertheless Ferron thinks he has observed that the disease is generally much more rapid in women arrived at the critical period of life than in others; Tartra says that women at this time are more subject to it.

Whatever may be thought of these assertions I have observed as follows:—Of twenty-three women who were asked in relation to the period at which the menses ceased, I found seventeen in whom the cataracts commenced five, ten, and even thirty years after this period. This result does not agree very well with Tartra's opinion. Five still menstruated at the time the disease began, and in one only did the disappearance of the menstrual discharge correspond with the commencement of cataract. But in this case the course of the disease was not rapid, as Ferron declares it to be in such a case, for at the end of a year the vision of the affected eye was still so good that the patient could easily have gone about without assistance even if the other eye had been wholly lost. At this time she was seized with erysipelas, and her face was enormously swollen. This affection lasted eighteen days, during which the cataract

made such progress that after this period the eye could no longer distinguish light from darkness.

As to hemorrhoids, out of thirty-three patients questioned upon this point, (twenty-eight men, five women,) twenty-eight had never had them, the five others (men) had had some or still continued to have them. In one only did they seem to have any connection with the cataracts. This was the case of a vine-dresser, aged fifty-two years, who had had very painful and often bleeding hemorrhoids during twenty years of his life; after which they gradually diminished and finally ceased. From that period he had observed a diminution in the strength of his eyesight.

IX. *Hereditary Influence.*—For a long time cataract has been thought to be hereditary, and examples are not rare of families many members of which are affected with it during two or three generations.

If the question of the hereditary nature of a disease be one of the most difficult to resolve *rigorously*, when it refers to diseases of extremely frequent occurrence, and which, without admitting any hereditary predisposition, but from the fact that they attack a great proportion of any given population, and consequently must frequently be met with in many members of the same family, this case is not the same with cataract, which, although not of rare occurrence, still is evidently not so frequently seen as phthisis, for example. Now it seems, to me at least, impossible not to believe that in many cases there is a common disposition among many members of a family which causes them to be affected with cataract simply because they belong to this family.

The following are my reasons for this opinion. Out of thirty-nine subjects (twenty-two men, seventeen women) who were able to give certain information with respect to this point, twenty-two assured me that none of their relations had ever experienced the slightest affection which could make any one suspect the existence of cataract: ten, on the contrary, (four men and six women,) declared that one or many members of their family had been affected with that disease; nearly all of them had undergone an operation, so that there could be no doubt in relation to the diagnosis.

While investigating this subject I learned the curious circumstance of a woman whose grandfather, uncle, two aunts, two cousins, (all on the father's side) had had cataract, and all had been operated upon. She herself, at the age of twenty, had been attacked with the same affection. Finally, out of four children which she had, one was born with cataract, discovered by M. Wenzel,—and, what is very remarkable, neither her father, mother, nor her sisters, had ever had any thing similar.[1]

Besides these ten evident cases, seven more patients said that one

[1] M. Roux operated for cataract upon three brothers, born in England, aged between thirty and forty. Their father and grandfather had suffered from the same disease, and they had a brother much younger than themselves in whom the disease had already commenced.

or many of their relations had lost their eyesight, and most frequently at an advanced period of life. It is probable that in some of them at least this must have been consequent upon opacity of the crystalline lens, but I was unable to determine accurately this point.

Thus, in at least a fourth part of the cases which I studied in reference to this subject, cataract had affected many members of the same family. It is impossible, as it seems to me, to see in these results nothing but the effect of chance.

X. *Blows, Exposure to the Sun, &c. &c.*—It is well known that some external injuries have been the occasional causes of cataract in certain cases of individuals predisposed to the disease. When the effect follows very soon after the cause, and the disease pursues a much more rapid course than usual, there is every reason for believing that it is not a simple coincidence. Many examples of this kind are given in the thesis written by Tartra. He relates, from Ferron, who was witness of the fact, the case of a lady whose eye was violently struck by the cork of a bottle, and on the next day the eye had a cataract upon it. A potter entered his furnace while it was still hot, and came out with two perfect cataracts. Fabricius Hildanus saw a lady, more than fifty years old, who became blind in one night by the formation of two cataracts, without pain or inflammation, but after having wept for many days for the death of one of her relations. A surgeon of Mayence relates the history of a man in whose eye cataract occurred immediately after a banquet, during which he became intoxicated. It is well known, likewise, that one of the accusers of the celebrated Dessault fell into a violent passion on hearing that he had been set at liberty. At that moment one of his eyes lost its power of vision, and the next day there was a cataract formed in it. Dr. Carron Duvillard observed in three persons, sixty years of age, cataract develope itself very rapidly in consequence of a blow from the cork of a bottle. At the end of two or three days the opacity was slight, but it became perfect during the course of the year; but the eye which had not been injured remained perfectly well. The same physician observed the same effects in two women between twenty-five and thirty years of age, one of whom had received a blow from a tennis bat, the other an injury from a billiard cue.

I have never seen a single case in which circumstances so striking as these just mentioned have preceded the formation of cataract. Of fifty-seven patients questioned in relation to this point, the only ones which presented any thing analogous were the following:—

1st. A woman, one of whose eyes was already affected with cataract, and the second began to lose its power of vision, after long-continued weeping; but one month afterwards the blindness had not made very great progress, and at this period the operation was performed.

2d. A man's sight began to fail one month after he had received a blow upon the eye, which had been sufficient to blacken the skin around the organ; but the progress of this cataract was slow.

3d. A third fell upon his loins, and, in consequence of this, was bled many times, and was confined to his bed for fifteen days; *two months afterwards*, he awakened one morning with his eyesight very indistinct, whilst it had been very clear on the preceding evening.

4th. A fourth was bitten near the eye by a horse, which caused such a severe wound that he was obliged to keep his bed for fifteen days; after which it *seemed* to him that the vision of this eye was *a little* less than natural, but the actual, or, at least, the evident commencement of the cataract did not take place until many years afterwards.

5th. A fifth, a director of the public coaches, having obstinately continued to look at the sun for some time, during a summer's day, one of his eyes began very soon to lose its sight, and some few days afterwards he could no longer distinguish light from darkness.

6th. Finally, a sixth received a severe blow, from a piece of wood, upon one of his eyes; it continued red and painful for four or five days; the sight grew feeble from that moment, and at the end of three or four months was entirely lost.

I have cited these examples, in order to show that the cases in which we can *rightly* attribute the cataract to an accidental cause must be rare, although certainly there is nothing more common than blows and wounds in the neighbourhood of the eye.

The fifth and sixth examples seem to me to be the only ones in which we may admit that the accident, which occurred shortly before the cataract began, may have had some influence in its production.

It must have been observed that, while reviewing the different circumstances which might be thought to be the causes of cataract, I have limited myself frequently to the simple announcement of the facts, without drawing any conclusions, either negative or positive. With respect to sex, age, hereditary influence, &c., the case was not the same, because on these points there was no chance of error; but should I be authorised to say, even if I had a very large number of cases, and those with strongly marked differences, that cataract coincides *peculiarly* with one or other temperament, with one colour of the eyes or hair, rather than another, and that it is especially apt to attack long-sighted individuals? Because a great number of those I have observed were cultivators of the soil, or old soldiers, ought I to infer that it attacks *chiefly* those who are constantly exposed to the effects of the sun, and changes of temperature? Certainly, I ought not to do so. All of these assertions may be true, but they are very far from being demonstrated.[1] It is not, in fact, from the absolute number of times that any circumstance presents itself before the commencement of a disease, that we must

[1] The fact of cataract being found in both sexes equally tends to prove that those are in error who think exposure to the full light of the sun, to a very hot fire, examination of very brilliant objects, &c., to be causes of the disease. It is, in fact, evident that women are much less exposed to these influences than men.

conclude that it is or is not a cause of such disease. We must likewise know whether, *cæteris paribus*, the same circumstance is not met with as often when there is no disease. Thus if, out of a given number of patients affected with cataract, there are three who have black to one who has white hair, forty long-sighted persons to nine short-sighted ones, &c. &c., I should be very careful of inferring that this disease occurs more frequently in those persons who have black hair than in those with white, in long-sighted than in short-sighted persons, &c. Who, in fact, can tell whether these are not the common ratios existing between the black and light-haired persons, long and short-sighted individuals? Hence, if this ratio should obtain among persons affected with cataract, it is evident that we can make no particular inference from it. It may even happen that the reverse of what seems to be the case at first may be the fact. For example, suppose, in a given population, six individuals having brown hair to one who has light hair; then, that, in a certain number of persons attacked with cataract, we find three having brown hair to one with light; we must rigorously infer that cataract attacks much more frequently people with light hair than others with brown.

These reflections are fully applicable to the study of the influence of professions. Who, in fact, can say whether the employment of a tiller of the ground is more liable than other business to produce cataract? Suppose that three or four cases of cataract are brought into the hospitals of Paris from the country, for one from the city, what does it prove, if the population which furnishes the former is three or four times greater than the latter; nay, perhaps, seven or eight times greater?

All these circumstances are of the greatest moment, and one is astonished to see some authors give a long list of *causes* without hesitation, and without, apparently, suspecting that they are neglecting some precautions which are absolutely indispensable! It is fortunate, moreover, when these pretended causes exist a certain number of times as simple coincidences, without any research being made as to their real influence! If the illustrious Corvisart, in this last respect, had simply analysed his own work, he would doubtless not have asserted that tailors are very subject to aneurism of the heart; for he would have seen that there is but *one observation* throughout the whole volume relative to one of this trade.

M. Parent Duchatelet, in his excellent work on the cause of ulcers, is the only one, at least to my knowledge, who has clearly pointed out, and sufficiently developed, this source of error; and he is very reserved in drawing his conclusions, and these are very limited. This is a misfortune; but it must happen frequently, so long as one prefers to give facts which are proved to be true, rather than mere possibilities, rigorous results rather than conclusions hazarded with more or less boldness. It is by this method, however, that we must proceed, without ever deviating from it, even if we should have the epithet applied to us, which is more witty than

just,—that there are some people who are so prudent that they are afraid to conclude that two and two make four.

Nevertheless, notwithstanding the insufficiency of all these data to enable us to arrive at any rigorous conclusions, I thought it improper to pass them by in silence. This species of investigation, notwithstanding its apparent sterility, is, in my opinion, of very great importance. What, in fact, is wanting for us to be able to deduce all the consequences contained in it? We need results of another kind, those of statistics, a precious science of recent origin, but which, on this account perhaps, is making rapid and sure advances to truths the most certain and the most useful. How many medical questions will admit of a rigorous scientific solution, after statistics shall have furnished the most necessary principles? Have we a right to dispense with its assistance, when we would study the influence of age, profession, habitation, kind of nourishment, habits, &c. &c.? Will it not be to a species of statistics which may be called physiological or pathological, that we must have recourse, if we would appreciate rigorously the influence of temperaments, of hereditary nature, of which so much is spoken, or even the effects of different diseases upon one another? Now all this belonging to the domain of observation, as it must necessarily be known at some time, is it not right to collect, on the other hand, materials which, although useless now, will not be less useful hereafter?

ARTICLE II.

COMMENCEMENT AND COURSE OF THE DISEASE.

The *commencement* of cataract may be slow or rapid; it may occur in both eyes simultaneously, or in one eye only; it may be sensible to the patients, or wholly unperceived, &c. Let us study these different features.

In the greater number of cases, cataract commences slowly; the patients perceive that their powers of vision are lessened, that they see as through a mist, and generally they perceive that this diminution is owing to an affection of one eye, while the other is perfectly well. This occurred in fifty-two out of sixty patients. In the remaining eight, to whom may be added two more who were operated upon in 1830, and who stated the fact before the question was asked, the commencement was more or less sudden. The first was a man, fifty-seven years old, clerk in an office. One night he went to bed, after having been engaged in his usual occupation, and without having perceived any diminution in the strength of the eyesight, and on the next morning, on awaking, he was hardly able to walk alone, both of his eyes being equally affected.

The second was a soldier, aged forty-two years, marching from

Valence to Paris, his sight being excellent, when one night, without any known cause, it became very indistinct.

A third, a labourer in the country, aged sixty-two years, perceived one morning that he saw things as if through a mist. He was certain that on the preceding evening nothing of the kind was present. Both eyes were equally affected, but it was not until after the lapse of two years that he became so blind as to be obliged to have some one to lead him.

The fourth, of whom I have already spoken in another place, a conductor of public coaches, aged sixty-five years, lost the sight of the left eye during the space of a few days, after having looked at the sun for some time.

The fifth was a soldier, aged fifty-nine years; the same effects took place in this case as in the third.

The sixth presented a very curious circumstance. It was the case of a day labourer, aged forty-seven years. Being in the street one day, he perceived for the first time that his eyesight was indistinct. He closed, alternately, his eyes, and discovered that the right was completely useless, whilst the left remained very good. Three years afterwards, while occupied in reading a journal, his second eye, which had not lost in the least its power, became suddenly so weak that he could not finish reading. Five or six weeks afterwards, he was so blind as to be unable to walk without assistance.

The seventh was an old captain in the forces of the Empire, aged sixty-two years. One morning he felt a very severe itching in one eye; it continued almost uninterruptedly for three days, and obliged him to rub it with great violence. After these three days, sight was wholly lost in this eye. The other did not begin to lose its powers until five years after, and in two months from that time, the sight was entirely gone.

The eighth was a porter, aged fifty-five years; one of his eyes had already for four years become gradually affected with cataract, but the other was so well that he was not troubled by it in the least. One Sunday, after having walked some distance, he went to bed with his eyesight as good as usual, and the next day he could not distinguish any thing more than light from darkness, and slightly the forms of larger bodies; but he was unable to go about without some guide.

Of twenty-six women reckoned among these sixty patients, two alone presented analogous peculiarities.

In one, aged thirty-five years, one eye had become gradually affected with cataract, and two years afterwards the second lost its powers rapidly, but the patient did not lose the power of walking without assistance until four or five months afterwards.

In the other, a sempstress, aged twenty-eight years, the first eye lost its powers in like manner gradually. Three years afterwards this woman having sewed all day, as usual, perceived that her eyesight failed rapidly; the next day she was unable to sew; three or four days afterwards she could not recognise her relations, and at the end of one week was unable to walk alone.

It is worthy of notice—

1st. Of these ten cases two only relate to women, although the number of these last is not very much less than that of men.

2d. Of these ten patients two only were more than sixty years old; four between fifty and sixty; four below fifty.

3d. In three cases, the commencement was sudden for both eyes at the same time; six times, on the contrary, one eye was lost suddenly; in the seventh case the disease began suddenly in both eyes, but at an interval of three years.

4th. This suddenness does not seem to have had any unfortunate influence upon the results of the operation, for out of fourteen eyes operated upon, nine were wholly restored.

Cataract commences generally in one eye. This occurred sixty-three times in seventy-two patients. The right eye was the first to be attacked thirty-five times; the left twenty-eight times. In nine cases, on the contrary, the two eyes began to grow weak at the same time.

Finally, it sometimes happens that subjects affected with cataract do not perceive in the least the loss of their first eye, but it is when accidentally closing the second that they perceive that the other which remains open has lost the power of vision. This commencement is, in fact, not very uncommon; for it occurred in a ninth part of the seventy-two patients. It is, moreover, a singular circumstance, and difficult of comprehension, that certain individuals perceive immediately a difficulty of vision from the fact that one of their eyes begins to grow weak, whilst there are other individuals who do not suspect that vision is entirely lost in one eye.

There is one subject the discussion of which naturally belongs to this place.

Can cataract *frequently* remain for many years, or even during life, confined to one eye, or is it the fact that, when one is thus diseased, there is a great chance that the second will be more or less speedily affected with the same disease?

The solution of this question, which at first sight seems merely one of curiosity, may become of the highest practical importance. If in fact, it were demonstrated that in certain cases we might prevent the formation of a second cataract or obtain the spontaneous cure of it at its commencement by operating upon the eye attacked first, we perceive that it would be very important not to attend to the rule generally given, that we should not operate until both eyes have cataracts formed in them. I know very well that this spontaneous cure of cataract is doubted by the majority of surgeons, but the fact is that operators have rarely been placed in favourable circumstances for determining the question, since there are very few who consent to have one eye operated upon before the second is affected. Opinion, moreover, is not decided upon the point. Thus Wenzel operated successfully upon a cataract in the right eye, and afterwards the patient refused to allow of a second operation upon the left. Twenty years afterwards the surgeon met the same individual, and was astonished to find that he saw equally well with

both eyes. He stated that the vision had *gradually* returned to the left eye without the employment of any remedies. The surgeon, however, observed that the left pupil was a little yellowish and opaque at its upper part. The same author is certain of having seen other similar facts. Saint Yves cites the case of a man wounded in the right eye; a short time afterwards this eye was affected with cataract, and the left one speedily underwent the same fate, but recovered *spontaneously* when the right eye had been operated upon. Finally, an English author of modern date, Mr. John Bower, in a work in which he proposes a new mode of operating, declares positively that one cataract cannot be operated upon too soon; since, says he, we may prevent the formation of a cataract in the second eye by operating early upon that of the first. In support of his opinion he cites the case of a priest whom he operated upon in Rome. The cataract was six or seven years old, and was of the right eye; an opacity was beginning to appear in the left. It disappeared wholly after this operation, which moreover was entirely successful. The same author quotes a passage from Wardrop which seems to confirm his opinion. "There is," says this last, "a frequent disease of the eyes in horses. It has the appearance of a specific inflammation; it attacks first one of the eyes, soon seizes upon the other, and sooner or later, wholly destroys the power of eyesight. Some veterinary surgeons know that if the eye first affected suppurates and is destroyed in its orbit, the disease does not attack the other, or it stops if it has already commenced there. Consequently they have adopted the method of wholly destroying the diseased eye in order to save the other. They succeed in doing so by piercing it with a nail, or by the introduction of lime between the eyelids."

I have myself never seen any thing in relation to the subject. I have alluded to the above mentioned statements merely to show that *it is possible;* at some future time it may be very important to know what chance there is of having two cataracts when there is one already.

I return to the latter question.

The answer commonly made is this. When one eye suffers from cataract the second eye *most commonly* becomes affected after a time with the same disease. I am far from denying the fact, but I contend that it is much harder to prove it than is generally thought. In fact, it may be said in opposition to this assertion, that if we see so many cases of double cataract in the hospitals and likewise among the higher classes, it is because, in the great majority of cases, those individuals who have one eye healthy do not ask assistance from surgery; and the following fact seems to support this view of the case, viz. those who have two cataracts rarely come to be operated upon until their eyesight is so diminished as to prevent them from engaging in their usual occupations; that is to say, very frequently they have a cataract of one eye for a longer or shorter time without thinking of attempting to rid themselves of it. Why may not this occur perhaps in a great number of indivi-

duals who may remain during a whole life with cataract of one eye?

I will now state what I have observed in reference to this subject, and the reasons which, in my mind at least, seem to prove that the general opinion is the correct one.

Of sixty-nine subjects who were admitted into La Charité during the autumn of 1832, and the spring of 1833, to be operated upon for cataract, *one alone* was attacked with this disease in one eye merely. The affection had existed for thirty years, but the patient had never thought of coming to get relief until slight obscurity, for a year previous, began in the other eye and injured its power of vision. Of fifty-two other patients who were operated upon, in 1830, ten had cataract in one eye only, but unfortunately I have not mentioned in my notes the state of the other eye. In *one only* is it expressly stated that one of the eyes was perfectly well, but the other had been affected with cataract *for a year only*. Something analogous occurred in one, and perhaps two more, but I must state that I do not know certainly in relation to this point. Thus, of a hundred and twenty-one patients, only two, it is certain, had but one cataract. Moreover, in only one was this disease of long standing.

But as the objection made already, occurs in this place, we must seek for other means of determining what is really the proportion existing between double and single cataracts.

Let us consider the following circumstances:

1st. Of seventy-two patients from whom I was able accurately to determine the commencement of the cataract, there were nine in whom this commencement was the same for the two eyes.

2d. In only eight of these seventy-two patients, the sight of one of the eyes was already lost, without the knowledge of the patients, when they perceived the second began to lose its power; consequently it is only among these eight patients that we can rigorously infer that the first cataract existed *alone* for a considerable time, a circumstance which is neither proved, nor does it seem probable, if we may trust the following facts:

3d. In all the remainder, the second cataract commenced some weeks, months, or at most, from three to five years after the first; in two only it commenced from ten to twenty-five years later.

4th. Finally, in forty-seven individuals, in whom I was able to determine, with some precision, the exact time that elapsed between the commencement of the first cataract and the moment when the patient ceased to be able to guide himself, the mean time, as I shall state hereafter, was only five years and one month.

If we consider all these facts we shall be led to infer that the first cataract (or, if the expression is liked better, the cause which produced it,) seems almost always to have had so close an influence upon the condition of the other eye, that there is but small probability of frequently meeting a single cataract for a long series of years; for whenever there have been two they have commonly succeeded each other after a short interval. Nevertheless, I allow that

this kind of indirect solution is far from being rigorously exact, for we must appeal directly to observation for this decision.

However this may be, there is one circumstance worthy of notice, viz: there is a very marked difference between cataract which, no one denies, attacks very frequently both eyes, and other diseases which seem to affect not to touch an organ when they have already attacked another which is similar. What is more rare, for example, than to see cancer attack both mammæ, or both testicles? Nevertheless, is there not as much similarity of organisation and of function between the two mammæ and the two testicles, as between the two crystalline lenses? and does not the cause which produces cancer, take as deep hold upon the frame when that cause is once developed, as that does which produces cataract?

Course of the Disease.—Very little remains to be said upon the course of the affection. Most generally it was slow and uniform, except in some cases which have been already quoted, in which it was as rapid as the commencement of the disease was sudden.

The patients perceived that the cloud which surrounded them gradually grew thicker, until they were unable any longer to walk without a guide. Sometimes only, the disease, after having commenced slowly, made rapid progress in a few days, or even in the space of one night. Thus, in a man, whose two eyes began to grow dim at fifty-nine years of age, the cataract went on so very slowly that, at the end of five or six years, he was still able to guide himself; then. without any obvious cause, during the space of one night, the blindness became complete. In one woman, of whom I have already spoken, an erysipelas of the face seemed to accelerate very much the course of the affection. Finally, in a third, the comencement and course of the two cataracts were slow. The patient still continued to see with one eye well enough to walk alone, when, after a long walk of five leagues, the disease went on so rapidly that, at the end of three days, it was necessary to conduct the person home, as she was unable to walk without some guide.

ARTICLE III.

SYMPTOMS AND DIAGNOSIS.

The symptoms of cataract are well known; nevertheless, as the exact value of some of them is not entirely decided upon, I will now examine them in detail. Besides those drawn from an inspection of the eye, there are others in the sensations experienced by the patients.

I. Thus, a uniform and constantly thick mist which envelops every object, and finally produces total blindness, is a constant sign in all individuals affected with cataract.

II. But in more than three quarters of the cases other sensations are combined with this. Thus, of fifty-nine patients with cata-

ract, thirteen only never perceived anything else than this uniform mist; the other forty-six, on the contrary, had many different illusions; they saw coloured riband, diamonds, flashes of light, metallic plates, bright rings, and snakes seemed to be moving before them, and this sometimes at the commencement in a transitory manner, at others at a more advanced period, and in some cases for many years in succession. But of all these illusions or imaginations, that which was by far the most common, was the frequent or perhaps constant apparent presence of one or many black flies, which seemed to be moving sometimes before one eye, at others before both eyes. In twenty-three, out of forty-six patients, this sensation existed either alone or combined with some others which have been already mentioned. I endeavoured to learn whether these last might not be owing to some spots or streaks which, as I shall hereafter mention, are seen rather frequently in the opacity of the pupil, but it was an explanation we were obliged soon to renounce, because two thirds of the patients who had these sensations had cataracts of a perfectly uniform appearance.

It would be very important to examine carefully in a large number of blind persons, all these alterations of vision, of which the cause is yet unknown; perhaps it would be the means of arriving at a better sign for recognising the disease, when the examination of the eyes is insufficient to remove all doubts. In fact, the opinions of authors are far from being unanimous upon the characteristic marks which distinguish amaurosis, for example, from cataract, in difficult cases. Professor Marjolin, for instance, in his article on amaurosis, in the Dictionary of Medicine, says that the fact of vision being performed better laterally than directly in front, is the peculiar symptom of cataract, since persons affected with amaurosis do not see any better when an object is in one place than when it is in another. On the contrary, Beer and Richter (quoted by Samuel Cooper) pretend that it frequently happens that in amaurosis a patient can see in some particular direction. Richter goes still farther. According to him, as it is the centre of the eye which is the part most frequently affected with gutta serena, it happens that *the majority* of patients who have commencing amaurosis always see better those objects which are situated laterally than those which are directly in front.

III. I cannot say what takes place in cases of amaurosis, as I have not seen a sufficient number of cases; but it is certain that it is not infrequent to meet, in cataract, with the symptom spoken of by Professor Marjolin, and this is well known. We must not suppose, however, that we meet with it very frequently. Many patients have never paid any attention to it, and can give no information respecting it. Out of forty-six patients affected with cataract, twenty-three were in this condition. Of the remaining twenty-three, fifteen had remarked that there had been a period at which they distinguished much more accurately the objects which had been placed a little on one side than those directly in front; eight, on the

contrary, stated that they had frequently tried every position possible, but had been unable to see better in one than in another.

IV. There is another sensation which has been said by authors to be peculiar to persons affected with commencing amaurosis, viz: they see any artificial light, any very bright object, surrounded apparently by an iris; in other words they are affected with diplopia. I know not what occurs in amaurosis, but these phenomena, at least, do not belong exclusively to this last disease, as they are met with, although rarely, in cataract. Thus, of twenty-five patients questioned in relation to this subject, three said that the artificial light appeared to them to be surrounded with the colours of the rainbow, and it is very certain that they were not amaurotic patients, for the extraction of the crystalline lens restored the sight, at least of one eye. In nearly all, the flame of a lamp seemed a very large sphere of light.

V. Nine experienced other sensations. Some, instead of one light, saw two or more; others again thought that they saw a great light, with many smaller lights issuing from it. In one, the flame of a lamp seemed composed of twenty or thirty lights when it was carried to a great distance, but it became single when near the eye. Finally, one, whenever he looked at the moon, thought he saw many luminous spheres, but an artificial light never produced the same effect.

It would be *curious* to discover whether the same sensations are found in amaurotic patients, and in what proportion.

VI. It is known that persons affected with cataract have one peculiarity, viz., they see better in a rather dark place than in another much lighted up, before sunrise and after sunset than whilst the sun is above the horizon. This is explained very well by the changes in the size of the pupil. In persons affected with cataract, as in those whose eyes are perfectly healthy, the pupil contracts on exposure to a somewhat intense light; but it dilates under opposite circumstances. Now, as the crystalline diminishes in thickness from the centre to the circumference, and the opacity of the cataract being commonly less at the circumference than at the central part of the lens; it is evident that images of bodies must come to the retina the more easily according as the luminous rays fall upon the crystalline nearer the circumference, that is according as the pupil is largely dilated. But this phenomenon of dilatation and contraction of the pupil, which is constant in persons affected with cataract, although affording an easy explanation of what is experienced when passing from a bright light to a rather dark place, seems to me to be a very strong objection to the common opinion, of the mutual dependence of the iris and retina. In fact, if it be true that in the healthy condition of the organ of vision, one of the functions of the iris is to close up the opening of the pupil in proportion to the quantity of light which the retina receives, in consequence of a kind of agreement, as it were, between these two membranes, the former regulating its motions always and solely according to the necessities and impressions of the second, how

does it happen that in cataract, a disease which does not change in the least the normal condition of the iris and retina, but which merely interposes between them an opaque veil—how does it happen, I ask, that this kind of agreement, this seeming intelligence of the iris, is so much perverted, that this membrane no longer regulates its motions by the sensations of the retina but simply by the quantity of light it receives itself, since at mid-day it contracts in such a manner as totally to deprive one of sight? This objection seems to me to be so important that until something farther is learned upon the subject I should prefer to admit that, in the healthy condition of the iris, it can move not merely under the influence of the retina, but likewise under the direct influence of the light which falls upon it.

This view of the subject explains what occurs not only in cases of cataract, but likewise in the healthy condition of the eye. I know very well that it may be urged that in amaurosis the iris is most frequently immovable, and seems, therefore, by this state of paralysis, to follow the palsied condition of the retina, but it seems to me that the objection is rather in the words than in the facts themselves. What in fact proves that amaurosis consists in a palsy of the retina, or even that the disease is seated in the retina? Absolutely nothing. We say that there is paralysis of the retina because we see all the tissues of the eye remaining transparent, while the eyesight is wholly lost. But why could not the cause of the disease have acted *primitively* as well upon the ophthalmic ganglion and the branches of the fifth pair as upon the other nervous portions of the organ of vision? Moreover, do we not see very frequently amaurosis as a symptom of an appreciable lesion of the substance of the brain or cerebellum without the least alteration in the texture of the retina or of the optic nerve? How can the retina in these cases cause paralysis of the iris, since it is in the nervous centre very evidently, and not in the retina, that the cause of this blindness exists?

Finally we need not see very many cases of amaurosis to be satisfied that the eyesight may be totally lost, while the iris may preserve perfectly all its motions. I would likewise remind the reader, as a proof of the possibility of the iris and retina being independent, of what happens when the extract of belladonna is applied to the eye. The pupil dilates, and remains immovable, and if the quantity applied be not sufficient to produce giddiness, the sensibility of the eye to light is not at all diminished; it is even a palliative means which has been advised in order to make vision more distinct in cases of commencing cataract.

I return to the subject which has drawn me into this digression. If in the majority of those affected with cataract the eyesight is clearer in rather dark places than in full light of day, especially at the commencement of the disease, this is by no means always the case. Thus, of fifty-five subjects, forty-five declared that, from the commencement of the disease they had been able to see better in a dark than in a very light place; in a few this difference ceased

after a certain time, or even the reverse took place, but the majority at the period of operation still presented this peculiarity. Of the remaining ten, four asserted that at no time had they seen better in the shade than in full light; six said that they had always seen better in full light or even when exposed to a bright sun that when in opposite circumstances. Though this last fact is difficult of comprehension it is not the less true, as I took great pains to ascertain it. It will, however, be soon seen that some doubts may be raised in regard to the simplicity of the cataract in two of these cases.

VII. *Mobility of the Pupil—State of the Eyesight.*—In all the patients who were operated upon, with the exception of a few cases which will be mentioned hereafter, all had the pupil regular, very movable, and the powers of vision were not so much destroyed but that the patients could distinguish light from darkness, and perceive very well when an opaque body passed between themselves and the light. Some of them were even able to see the outlines of large objects and to recognise bright colours.

The following were the exceptions:—

1st. In one man, aged sixty-four years, the pupils were about half a line in diameter; they were very slightly irregular, their powers of motion very doubtful, nevertheless the patient distinguished very easily light from darkness, and even the forms of bodies. He had always been able to see better in a dark place than in one that was very light, and he had never experienced any pains in the head. One eye was operated upon; no adhesion hindered the crystalline lens from coming out; the patient was able to distinguish the full light of day, but the eye was destroyed by suppuration. It appears very probable, notwithstanding the condition of the pupils, that it was not a case of amaurosis.

2d. The following fact presents very different circumstances. It relates to a man forty-five years of age, of whom we have already spoken. For thirty years at least his right eye was able to distinguish merely light from darkness, and not better in the evening than in the middle of the day; he could recollect nothing at all in reference to the origin of the disease, but he simply declared that he had never had pain in the head; the pupil was of regular shape, from one to two lines in diameter, very movable; the cataract of a uniform milky white appearance. The other eye was perfectly healthy, excepting some slight spots upon it of recent origin. This man was operated upon, and after the incision into the cornea a small quantity of the vitreous humour, in a tolerably fluid state, flowed out; the operation was suspended, and no accident occurred. A month afterwards the extraction was again attempted. Immediately after the section of the cornea the aqueous humour came away as usual, but rather more viscid than usual. It was evident that it held in solution a small quantity of the vitreous humour; nevertheless the capsule of the lens being opened with a needle the cataract immediately flowed out in the form of a milky fluid; the pupil remained perfectly black, but the eyesight did not gain any even at

the moment. Seven days afterwards, the cornea and pupil were very distinct, the iris had the power of contracting, the patient was able to say whether the fingers which were held up before him were closed or separated from each other. In a short time this power was lost without any pain, and when the man left the hospital, fifteen days afterwards, he had in the field of the pupil a grayish triangular spot which occupied hardly a third part of it. The rest of the opening was very black; the patient could not perceive any thing but the daylight, but the iris preserved its contractility. This case is of a nature to leave us in some doubt upon the integrity of the retina, and of the vitreous humour before the operation.

3d. The third case relates to a man seventy-two years of age, not subject to cephalalgia. His eyesight had been diminishing for several years, but he could not precisely fix upon the epoch at which this affection began. From that period he had always seen better in the dark than in the sun-light; the pupils were regular, about a line and a half in diameter; they *contracted but very little.* The patient was unable to walk without a guide, but he distinguished still with accuracy the forms of bodies. Extraction was performed on one eye; the patient immediately saw the objects which surrounded him. Unfortunately, inflammation commenced, and the cornea became opaque. Notwithstanding the slight degree of mobility of the pupils, I think it cannot be admitted that this cataract was complicated.

4th. The fourth case is one of the most remarkable. It relates to a man forty-five years old, of whom we have already spoken, and in whom the sight of both eyes was suddenly destroyed—that of the second while he was reading a journal. When he came to La Charité, the pupils were very much contracted—certainly not more than a line in diameter: the left regular, but having very little power of motion; and, by means of this eye, he was able to distinguish light from darkness, but nothing more. The right pupil was rather oval from above downwards, and completely immovable. The right eye was incapable of receiving impressions from even the daylight; the cataracts were brilliant, of a pearly gray colour. Professor Roux operated upon the left eye; and immediately after the operation the patient saw his fingers with so much distinctness, and he begged so earnestly to be operated upon in the other eye, that M. Roux finally consented. Notwithstanding the small size and immobility of the pupil, no adhesion prevented the extraction from being performed; and immediately the patient assured us that he saw his fingers, though less distinctly than with the other eye. One month afterwards he quitted the hospital in the following condition:—In the left eye the cornea was very clear, although the flap was a little prominent; the pupil was very black, immovable, rounded above, irregular below, and extending like a depression as far as the cicatrix on the cornea, to which the iris adhered over a small extent of surface. With this eye the patient recognised very easily a watch, and distinguished even the hands upon it, although he did so with some difficulty; but it seemed to

him as if a spider's web were stretched constantly before him. In the right eye the cornea was clear, the pupil quite narrow, not exactly round, perfectly black and immovable. With this eye the patient was able to see more distinctly than with the other; it seemed to him as if he would be able to read large letters; he congratulated himself very much on the success of this second operation. Finally, his eyesight had this peculiarity about it—it was very short; and when he walked alone he was obliged to use great precaution, for he could see only a few paces from him.

5th. The following case is interesting under other points of view. It relates to a man aged fifty years. When splitting wood, in the winter of 1831, a piece struck his eye; the part remained red and painful for four or five days; from that time the sight of this eye diminished, and in three or four months was wholly gone. The right eye began to lose its power as soon as the left was lost, and in about the same length of time was in a similar condition. The patient had never been subject to headache; had never been able to see better in the evening than at mid-day; on the contrary, at the time when he could see enough to enable him to walk alone, he preferred a bright light. When he came to La Charité, his pupils contracted but slowly and very slightly. The right one was rounded, and of a regular shape; the left extended a little towards the outside, in a kind of oval depression. The irides seemed a little prominent in front; the cataracts were of a grayish-white colour, studded with bright specks, and clouded as it were; the patient was able to distinguish light from darkness, but nothing more. Extraction was performed on both eyes, and nothing peculiar happened, save that in the right there was a slight lesion of the iris at its external part. The patient quitted the hospital seven weeks after the operation, in the following condition:—Both corneæ were perfectly clear; the pupils very black—the right cicatrised below and towards the outside, the left rounded and about two lines in diameter; both appeared motionless, and the patient scarcely perceived the light of day, much less than before the operation.

Was there any disease of the nerves of the eye at the time this patient entered La Charité, or did not this affection develop itself until after the operation was performed? I cannot answer the question. The previous history proves that we cannot declare that amaurosis complicates the cataract, either from the slight mobility or irregularity of the pupil, or even the almost total inability to see the light. In the last patient, it is true that there were some pains after the operation; the right eye especially was the seat of frequent piercing pains; the conjunctiva was red; there was pain in the forehead and teeth of the upper jaw, but all this was confined to the right side; and, admitting that these symptoms might explain the loss of the right eye—which, however, presented no appearance of internal inflammation—how can we explain the loss of the left, which experienced only a few pricking pains?[1]

[1] I heard M. Roux say, in relation to this patient and to the one mentioned in the second observation, that he had sometimes seen patients, upon

6th. An unmarried woman, aged fifty, had two cataracts. The sight of the left eye had gradually decreased without any headache, and at the end of four years the patient could no longer walk without a guide; the right eye grew weak a short time afterwards, and had arrived at the same state with the other in twenty-one months. The left pupil was a little irregular, *immovable*, about one line in diameter; with the corresponding eye the patient could distinguish light from darkness. The pupil of the right eye was very regular, and sensible to the action of light; but with this eye the patient saw rather less than with the left, and likewise better towards evening than in the middle of the day. Extraction was performed on both eyes; there was no adhesion in the left, notwithstanding the contraction and irregularity of the pupil. Unfortunately, both eyes were destroyed by suppuration, so that we cannot say whether the condition of the left pupil coincided with a lesion in the functions of the retina. But, at least, it results from this last case, and from

whom had been performed the operation of extraction, present, without any previous evil consequences from the operation, the following condition, viz:—the eye seemed perfectly healthy, and perceived the effect of the light, but was unable to distinguish any objects. Now, according to him, we may explain the phenomenon in the following way. We see bodies distinctly only by means of the convexity of the crystalline lens, which collects the rays upon the bottom of the eye. Suppose, now, that there is a concave surface instead of a convex one to the crystalline—for example, the anterior depression of the vitreous humour—the luminous rays will be refracted, it is true, in passing from the aqueous humour into the vitreous, and will be drawn towards a perpendicular raised at the point of incidence; but as all these perpendiculars themselves diverge from before backwards, the luminous rays will diverge likewise, and will be dispersed instead of forming distinct images.

Now, if the eyesight is regained after the displacement or extraction of the crystalline lens, it is because the vitreous humour projects forwards, and takes more or less the place of the lens; but if, for any reason whatsoever, the vitreous humour keeps, after the operation for cataract, the concave form which is peculiar to it in front, what has just been stated will inevitably happen: there will be some sensation of light, but no distinct images.

This explanation, it will be perceived, is very ingenious, but it needs direct demonstration by facts. Now we find, in rather an old author, the account of a dissection very well made, which confirms at least one half of Prof. Roux's theory. It relates to a poor woman, for whom Maitre-Jean depressed two cataracts during the year 1691. The operation succeeded in both cases; ten days afterwards the patient saw well enough to be able to *pick hemp*. One month afterwards she died of pneumonia. The dissection of the two eyes was made some hours after death, and the writer speaks thus of the vitreous humour:—"I had the pleasure of remarking, when looking through the pupil, that the crystalline lens was no longer in the place which it usually occupies, which is in the middle of the anterior part of the vitreous humour. This part of the vitreous humour formed *a very smooth and even prominence, which resembled the anterior surface of a crystalline lens*, save that it had no depression in it; and when I pressed gently into it with a probe, it arose immediately upon the pressure being removed, and resumed its previous form."

Nothing can be more clear than this description. It remains now to demonstrate that it is always thus when the operation is successful, and that in certain unfavourable cases the change of form does not take place.

the fourth, that contraction, irregularity, and immobility of the pupil may exist without the least adhesion of the iris to the crystalline capsule, since there was no difficulty experienced in extracting the lens.

In order to terminate what I have to say upon the symptoms, it remains for me to speak of *the aspect of the pupil* itself, or of the cataract before the operation.

VIII. All cataracts do not have the same appearance; nothing, on the contrary, is more various, when we examine them closely and carefully. The varieties refer to the colour of the opacity, its more or less dull or brilliant appearance, and the uniformity or non-uniformity of its colour. It is difficult to make a small classification of all these different appearances; however, it is possible to do so, by following, in some measure, an arbitrary rule, which we can hardly avoid doing. Let us examine this subject.

The appearance of one hundred and nineteen cataracts was described carefully in sixty-five patients. The cataract of one eye differed sensibly from that of the other eye in three fifths of the subjects who had two. We may refer to the four following shades those which these cataracts presented.

1st. *Gray.*—I understand by this term, all those tints in which gray predominates, and in which there is not any blue reflected. This colour was of more frequent occurrence than all the rest, for it was observed in forty-seven cataracts,

2d. *White.*—Namely, the milky white, opal, a little yellowish, pearly, &c., but without any bluish tint—forty-one cataracts.

3d. *Bluish.*—This class contains the gray or white tints, with a bluish reflection that is very evident—twenty-two cataracts.

4th. *Blackish.*—I do not mean black cataracts, properly so called; I have met with none; but simply those which resemble in their hue a gray, or a very deep brown colour—five cataracts.

Finally, we must add to these cases one in which two cataracts had a very peculiar appearance. They occurred in a woman, aged thirty-five years. Their chief tint was brownish, but at the centre of each pupil there was a little white spot, almost exactly square. It was owing to a partial opacity of the anterior part of the crystalline capsule, and it was found necessary to remove it by means of small forceps, after the lens was extracted.

Besides the colour, the more or less uniform or spotted appearance of the cataract is an important point to be considered. The latter appearance is not uncommon, for it was found on twenty-six of the sixty-five patients of whom we are now treating, most generally in one eye, seven times only on both eyes. In all these cases we perceived in the pupil either simple points, not quite so clear as the rest, or of a different tint, or, perhaps, ill-defined spots, having a cloudy or pearly aspect. All this was very distinct from the opaque crystalline, on which they were situated. In one case, three very fine gray lines radiated from the centre of a cataract of a uniform pearly colour, and extended towards the circumference,

dividing thus the crystalline into three segments. This case resembles the species of cataract described by Professor J. Cloquet.

From the earliest times, much has been said of the great importance to be attached to the colour, and more or less uniform appearance of the cataract. Maitre Jean treated of it at some length, in reference to prognosis; he gives a list of good and bad colours; the irregularity of colour is, in his opinion, a suspicious circumstance. At the present time, we no longer dispute about this point; but distinguished surgeons and oculists seem to believe, that by the appearance we can decide whether a cataract is capsular or lenticular, soft or hard. According to Beer and Travers,[1] in membranous cataract, the colour is always clear, *never uniform*, but it is *spotted* or *streaked*. According to Beer, the caseous cataract is never of a uniform colour, but is always more or less spotted, the spots having a pearly aspect. It is true that Travers advances a different opinion upon this subject; he thinks that the caseous cataract is always opaque.

It would doubtless be very advantageous to know, before performing an operation, what kind of cataract we are about to operate upon, and especially whether or not it be a membranous one, for we increase the chances of evacuating the vitreous humour, when we attempt to draw from the eye an opaque capsule, after we have extracted the lens. Unfortunately, and notwithstanding the authority of the able writers to whom I have alluded above, the diagnosis is most commonly impossible, at least if we take the symptoms mentioned by these authors. I shall prove this hereafter, in a more detailed manner.

But before examining this question, there is another which is very important, and which presents itself naturally in this place, because it is intimately connected with the former.

Are capsular cataracts of frequent occurrence?

Many authors who have written upon diseases of the eyes, have not alluded to this subject; others have not hesitated to say that *anterior capsular cataract is by no means rare* (S. Cooper). According to Tenon, *facts and reason prove that it is not a rare circumstance to see cataract depend upon opacity of the capsule.* It is true that a little farther on he satisfies himself with saying, that cataract depends *sometimes* upon the membrane enveloping the crystalline lens. Dupuytren goes still farther. In the General Repertory of Anatomy and Natural Philosophy, vol. 3, second quarterly part, is a very interesting article upon the operations for cataract performed at Hôtel-Dieu, in which the author makes Dupuytren say that capsular cataract is common (*i. e.* that of the lens), in the proportion of one to one and a half; and this assertion has been repeated in the lectures (*leçons orales*) of the professor. I confess that I can scarcely help fearing that there may be some mistake of the press, or of the writer, in these passages. In fact, when studying

[1] Samuel Cooper's Surgical Dictionary.

the table of two hundred and seven patients with cataract, who came to Hôtel-Dieu, from 1815 to 1821, and upon whom two hundred and sixty-four operations were performed, (extraction only eight times,) we find these words, "*membranous cataract*," applied to each one of the cases where this kind of cataract had been apparently diagnosed. Now, how many times is membranous cataract spoken of? Fifteen times only in two hundred and sixty-four, which gives a proportion of one to sixteen and three fifths, instead of one to one and a half. The difference we readily see is very great! Whence arose the error? Is it owing to the omission of a great number of membranous cataracts, which were not noticed in the table of which I have spoken? Or must it be in the proportion of one to one and a half, which the author of the article attributes to Professor Dupuytren? This is the difficulty. I will attempt to solve it by examining another series of facts.

If some doubt may remain in many cases, whether the cataract be owing to the capsule, or to the lens, when the operation of depression, or of breaking up of the lens, is performed, no doubt can remain after extraction. It is the very *touchstone* of diagnosis. In fact, after the removal of the lens, the pupil remains either perfectly black, or more or less opaque. In this last case, no doubt can be felt in relation to the total or partial non-transparence of the anterior capsule; for we are then obliged to remove it by means of small forceps, or cataract needle.

Now, out of one hundred and twenty-one subjects, operated upon at La Charité, extraction was performed one hundred and seventy-nine times, and in this number, *five times only* was the capsule of the lens evidently opaque, which gives one to thirty-four and four fifths, instead of one to one and a half, for the proportion of membranous to lenticular cataracts. I say *evidently opaque*. In fact, there was a certain number of cases in which the pupil was not very clear after the extraction of the lens, either from the fact that the curette was not introduced for the purpose of taking away some remains of the cataract, or because in some of the cases there was a commencing opacity of the capsule, and it was not detached. These cases were six in number. Let it be understood, that I now speak only of those in which the operation was unsuccessful, for it is very evident that I ought not to allow that there *might have been* capsular cataracts, in which there remained in the pupil, soon after the operation, some slight opacity, which was afterwards completely dissipated by the absorption of the remains of the cataract to which it was owing. Thus admitting, as demonstrated (what is very far from being actually the case), that these six cases, of which I have spoken, were *all* cases of membranous cataract, there would be eleven in one hundred and seventy-nine; which would give a proportion of one to sixteen and three elevenths, which is still very different from the proportion of one to one and a half.

What inference should we draw from this? It is this, that it is very probable that, from some error of the writer, or of the printer, the illustrious surgeon of the Hôtel-Dieu is made to say, in the

article of the Repertory above alluded to, and afterwards, in his lectures, that membranous cataract is to cataract of the lens as one to one and a half. It is evidently much more rare.

It seems scarcely necessary to make any remarks upon the diagnosis of capsular cataracts; it is sufficient merely to remember that there were five only which were evidently of this latter species, while in thirty-three cases the cataracts presented a spotted or clouded appearance. These facts are sufficient to show how very little confidence we can put in this sign, which, according to Beer and Travers, is sufficient for the diagnosis.

In these five cases, the aspect of the cataract, before the operation, was as follows:—

1st. In an unmarried female, twenty-six years old, the pupil was of a very pure, *unspotted* white colour; after the extraction, this same colour continued; the anterior part of the capsule was removed by means of a needle, and immediately afterwards the opening of the pupil became perfectly black.

2d. In a man, aged sixty-five years, the cataract of the left eye was of a beautiful brilliant white colour, with some spots which seemed still whiter. After the operation, there remained in the field of the pupil a semi-opaque pellicle, which was removed with the needle.

3d. In a man, aged sixty-six years, the two cataracts had a uniform bluish tint, with some very minute black streaks irregularly placed about their circumference, which seemed to be owing to some pigment. After the removal of the crystalline of the right eye, the pupil remained very clear, with the exception of a minute white speck which remained on its inferior portion, and was not removed. Forty days afterwards, the patient quitted the hospital; the pupil was clear, and the same white speck continued as on the first day.

4th. The fourth case was that of a woman of whom we have already spoken, and who had, in the centre of each pupil, a small square white spot, of very remarkable appearance, and situated upon a brownish base.

We see that, except in this latter case, the opacity of the capsule presented no peculiar or characteristic appearance.

ARTICLE IV.

DURATION.

Before passing to what relates to the operation itself, it remains for me to speak of the formation of cataract, with respect to the time that elapses between the first moment at which the patient perceives the eyesight to begin to be weak, and that at which he is unable any longer to walk without assistance. It is a subject

which is somewhat difficult of investigation, or rather it will require some length of time to examine it. In fact, we must give up all hope of obtaining any information on this subject from a number of patients, who are so thoughtless of what happens to them, that they were unable to fix, in a precise manner, the epoch at which they remarked for the first time that their eyesight was less powerful than previously; sometimes even the period when they lost the power of conducting themselves. But this is not the case with all; many remember all these different periods; they can tell the month, the circumstances under which they first observed the difficuty of vision; they follow anxiously the progress of their disease, and mark with great precision, for each eye, the time which elapses between the period of commencement and that at which the sight is wholly destroyed. It is from the results furnished by this last class of patients, unfortunately but too small, that I shall draw my data at the present time.

In forty-seven patients, I was able to determine the time which elapsed between the apparent commencement of the cataract and the period at which the patients lost all power of guiding themselves. The mean time was five years, one month, and about four days. But by separating that which has reference to men (twenty-five in number) from that relating to women (twenty-two), the mean is found for the former to be four years, two months and about twenty-three days, and for the women, six years and one month, minus a few days. It is true that among the latter were some in whom the disease went on so slowly (from fourteen to thirty-one years in duration), and they were such exceptional cases, that they alone augmented very much the mean number. However, omitting these, the mean time for females still continues at about four years, five months and twenty days; that is to say, three months longer than that of the men.

These durations were as follows:—[1]

[1] There is one precaution to be observed when we seek for the mean durations, viz., we should indicate, when it is possible to do so, into how many principal groups the objects which we study can be divided. Unless we do this, we are liable not indeed to obtain false results, for a numerical result is, speaking generally, always true if the data on which we proceed are true, and if no errors have been committed while investigating the numbers, but we may obtain results which do not represent every thing that we would wish to know. I will cite an example in explanation of my assertion. Let us suppose this very general question; to determine the mean age at which cerebral affections develope themselves. If it is true, that it is at the two extremes of life that we meet most frequently these diseases, it must follow that, if we collect a certain number of cases, we shall have two groups, one of children the other of old persons. If, now, we add their different ages, and divide the sum by the number of individuals, we run the risk of obtaining a mean age, which would certainly be very correct (thirty years, for example), but which would teach us absolutely nothing; nay, it would, in certain respects, lead us into error, since it would represent precisely an age at which we meet very few cerebral affections, instead of that at which they appear the most frequently. It will be easily seen that we need only two lines of explanation, and the obtaining of two mean times instead of one, in order

MEN.

Two men, in whom, from the commencement until complete loss of eyesight, less than one year elapsed.

16 do. from 1 to 5 years.
7 " " 5 " 10 "

WOMEN.

0 below 1 year's duration.
13 from 1 to 5 years.
7 " 5 " 10 "
2 " 10 " 31 "

Let us now see whether the disease is equally long for the first and second eye that is affected.

Thirty-six patients were able to tell me how long a time elapsed before the first eye was completely blinded by cataract. The mean number expressing this time is about two years five months and ten days. Separating the nineteen males from the seventeen females, the mean time becomes one year seven months and a quarter for the former, two years and seven days for the second (I do not include in this calculation the two females of whom I spoke previously). These durations may be arranged as follows:—

Eye lost almost suddenly, in	1	patient.
" after some days,	2	"
" between three months and one year,	17	"
" " one year and five years,	14	"
" " eight and twenty-five years,	2	"

The following are the analogous results furnished by forty-five patients, upon the duration of the course of the disease in the second eye that is affected. The mean number, expressive of this duration, is one year eight months and a half. Separating males from females, we have, for twenty-four men, one year four months and two thirds, and for twenty-one women, two years and fourteen days.

Uniting the two sexes, we find that the second eye was lost,

Between one and eight days, in	2	patients.
" one month and one year,	23	"
" one year and five years,	17	"
" five years and nine years.	3	"

These numbers are too small for us to infer from them the influence which age may have upon the more or less rapid progress of the disease, but they indicate a somewhat important differ-

to obviate this inconvenience, and obtain precisely what we seek for. This, by the way, is no objection to the use of the numerical data, but it teaches us merely that we should employ them properly, and as much may be said of every good thing.

ence in consequence of sex, upon which I shall speak hereafter. It results from these mean terms that the two eyes do not, in general, lose their sight with equal rapidity, and that it is particularly the first which becomes diseased whose decrease in the powers of vision is the most gradual. This is not, however, always the case. In fact, out of thirty-two patients affected with cataract, there are two in whom the sight of both eyes continued equally long before its abolition; twenty-one in whom this time was longer for the eye that was first affected than for the second; nine in whom the reverse took place; and in these last thirty subjects, the difference between the eyes was such, that their mean was two years one month and about twelve days.

We should observe that the mean (five years, one month and four days) which indicates the total duration of the disease until complete maturity of the two cataracts, is greater than the sum of the means which represent the time which each cataract has occupied in arriving at the same point; this sum, in fact, would be only four years and about two months. This happens from the fact, that, in a certain number of individuals, some time elapses between the moment at which the first cataract is completed and that at which the second begins.

The following facts occurred in thirty-two patients (eighteen men and fourteen women), in whom the cataracts did not commence at the same time. Of the eighteen men, there were thirteen who, having one eye completely blinded by cataract, have yet kept the other eye perfectly healthy, generally, for a long time. Thus, in two only out of these thirteen did the other eye remain well for less than one year; in the other eleven, it continued so for one, two, four, five, and even seven years. In the last five, the second eye was attacked before the complete loss of the first. With respect to women, the facts were entirely the reverse. Out of fourteen, five kept their second eye in a perfectly healthy condition from four to six months after the loss of the first; two only had it well for twelve and eighteen months. In the other seven, the second eye became decreased before the first was wholly blind.

In conclusion,—

1st. Considering the disease without reference to sex, the mean duration of the disease, counting from the apparent commencement of the cataract until the epoch at which the patients were unable to go without a guide, was five years one month and about four days.

2d. Thirty times in thirty-two cases the cataract which commenced first occupied, before arriving at maturity, a different length of time from that taken by the other cataract to arrive at the same degree of development; and twenty-one times in thirty the first eye that was affected was that in which the disease went on the most slowly.

3d. The mean time for the complete formation of cataract was, for the first eye, two years five months and a third, and for the second, one year eight months and a half.

With respect to sex,—

4th. The total duration of the disease was longer in women than men.[1]

5th. This longer duration is observed likewise in women, when we examine the course of each cataract separately.

6th. Thirteen men out of eighteen, and only seven women in fourteen, had the second eye perfectly well for a certain time after the loss of the first; and, in general, among the men, this time was much longer than among the women. We shall hereafter see that in the latter the prognosis is less favourable than for the former.

These various results seem to me to be sufficiently interesting to induce us to make further researches. I am far from wishing to affix to them any value which does not rightfully belong to them; I know that the numbers upon which they rest are too small for us to depend upon them as perfectly exact; I cannot disguise the fact that, notwithstanding I was as accurate as I was capable of being in collecting my data, some errors of date may have occurred which would be of very little importance if the numbers were large, and might be of great moment under the contrary circumstances. Still, I think that this subject, being important in the history of cataract, and having been studied as yet but little, at least to my knowledge, deserves to be re-examined with care. It belongs to observations to be made hereafter to confirm or overthrow these data which I have given.

ARTICLE V.

I now pass to the consideration of the operation, its difficulties, the accidents liable to occur in consequence of it, and its results.

OPERATION.

Extraction is the method which M. Roux prefers, and almost always employs. I have seen him make use of the needle in only seven cases, and they were those in which it would have been either extremely difficult or impossible to extract, without injuring the cornea; for example, because of the extreme depression of the globe of the eye, its small size, its constant motion, &c. &c.

The reader will easily understand the impossibility of my even thinking to discuss the question as to what is the preferable mode of operating, or whether there is any one preferable to another. I make this remark certainly not because this question may seem to be an idle one, or because it has been sufficiently decided by the

[1] This result is confirmed, if we compare males with females in eight subjects, in whom the two cataracts commenced at the same time. In fact, the mean duration of their formation was three years four months and a half for the five men, four years one month and a half for the three women.

contradictory assertions of authors, but because it is, I think, very difficult, perhaps impossible, to be decided upon, if, at least, we wish to compare the value of the methods themselves, employed in analogous circumstances, and with all the perfection of which they are capable of being performed, rather than the talents of the operators. In fact, we see that, in this question, the mode of performing the operation is not a circumstance of little moment; and because one surgeon obtains more successful results, by depressing or breaking up of the cataract, than another, or even he himself does by extracting, it by no means follows that the difference depends upon superiority of the former method over the latter. What more is necessary for the solution of this question? We must know, among all surgeons, him who performs the greatest number of successful operations by means of extraction, and compare these results with those furnished by depression, when practised in the most favourable manner. What immense difficulties!

It is only in the spring and autumn, as I have already stated, that patients are admitted into and operated upon at the hospital of La Charité. From the time of their entrance, they are obliged to make use of whey and pediluvia, with mustard in them. This is the chief treatment. It is very seldom that recourse is had to evacuations of blood. On the morning that the operation is performed, a blister is applied to the back of the neck, and it is kept in a suppurating condition until the patient leaves. Never, or almost never, does M. Roux use any means for dilating the pupil.

The operation is always performed in the amphitheatre, and if there is a double cataract both eyes are operated upon the same day. The patient is seated directly in front of the window, upon a seat which is rather lower than that upon which the operator sits. One eye is kept shut by means of a compress; his head rests against the breast of an assistant, who raises with the index finger the upper lid of the other eye, so that the pulp of this finger touches and even compresses slightly the globe of the eye towards its inner part. The operator depresses the lower eyelid and likewise touches with the index finger the internal part of the globe of the eye, by which the motions of the organ inward are at least partially prevented. Holding in the other hand Richter's triangular scalpel, he directs its point, at a sufficiently open angle, upon the edge of the transparent cornea at a little distance from its union with the sclerotic. It is always much above the external extremity of the transverse diameter of the eye, that Professor Roux makes this puncture; but the exact height varies. When the eye is properly disposed for it, it is such that the incision is as much external as inferior, and in this way the directions given by Wenzel are precisely followed. When once the cornea has been pierced, the blade of the instrument is carried in a direction parallel to the iris, until its extremity comes out at a place almost diametrically opposite to that by which it entered. However, this second puncture is almost always so directed that when the section of the parts is completed it comprehends a little more than half the circumfe-

rence of the cornea. It is impossible for me to say that this amounted to the seven twelfths rather than the nine sixteenths, and as the difference between these two fractions is only one forty-eighth, there is a great chance, I think, of the rules given upon this subject by many authors being never followed exactly. Finally, as soon as the point of the scalpel appears at the internal angle of the eye, the fingers of the operator and of his assistant must cease to press, in the least, upon the organ, and the section is performed by pushing the blade along parallel to the iris, and in the same direction as before, but without ever pressing from above downwards.[1] This mode of using the scalpel is so important, if we wish to obtain a smooth cut, that Professor Roux always does so even at the risk of pricking the skin, or the conjunctiva of the internal angle of the eyelid, which accident happens not unfrequently, and upon it I shall make some remarks at a future time.

This first part of the operation having been performed, the eye is allowed to remain quiet for some moments, then the assistant raises again the upper eyelid, taking great care not to compress at all the globe of the eye, and the operator, armed with a cystotome having a blunt and convex back, and a concave cutting edge, raises the flap of the cornea with the back of the instrument, makes the point penetrate with precaution the posterior chamber, and taking care not to thrust it deep enough to injure the cells of the vitreous humour, he pierces with one blow the double membrane covering the crystalline, by making in it a little incision which resembles, except in size, that of the cornea. He then withdraws the instrument and applies, horizontally, the little curette which is at the other extremity of the instrument, upon the skin of the upper eyelid very near the brow, and opens, very gradually, the eye by pressing lightly upon its superior part. This manœuvre causes the lens to make half a motion of rotation which carries backward its upper part and throws forward the lower. This last falls into the opening of the capsule, traverses the pupil, draws along with it sometimes the lower edge of the iris, raises the flap of the cornea, and slowly protrudes through the aperture in this membrane. As the cataract advances the surgeon diminishes the pressure which he exerts upon the globe of the eye, so that at the moment that the transverse diameter of the lens is in contact with the two angles of the incision of the cornea, he merely supports the lid and allows this species of delivery, as it were, to terminate spontaneously. All these minute precautions, that especially of terminating slowly the different periods of the operation, among others the section of the cornea, and the extraction of the lens, are of very great importance if we would avoid a grave accident, the protrusion of more or less of the vitreous humour.

Immediately after the operation the eyes are covered with a large

[1] An English author by the name of Wathen advises, very improperly in my opinion, to finish the section of the cornea by pressing the scalpel from above downwards.

compress of white linen, and over that is put a band of black silk, and the whole is confined by means of pins to a cap. This arrangement is not disturbed for five days; but from this period the patients wash their eyes several times a day with cold or warm water, mixed with a weak solution of the acetate of lead. The first day they are obliged to adhere to a very strict diet; from the second to the fifth a little beef-tea is allowed; afterwards some soup, and finally half or three quarters of a loaf of bread.

As there is no room at La Charité particularly devoted to diseases of the eye, the beds of those who have been operated upon are necessarily surrounded by a thick cloth, in order to prevent the full light of day coming to the patients. No one can conceive of the disagreeable confinement produced by this method, which prevents all circulation of the air; and it is not impossible that this circumstance may have some effect in causing a failure in the success of the operation. I should likewise state that women, in order to return to their beds, are obliged to go up a flight of steps communicating with the external atmosphere by means of a door that is always open.

DIFFICULTIES AND ACCIDENTS RESULTING FROM THE OPERATION.

But the operation is not always terminated thus simply; some *difficulties* occasionally make it longer, and *accidents* may complicate it. I will now speak of both of these.

1st. The mobility of the globe of the eye, the little command which certain patients appear to have over the muscles of this organ, render the puncture of the cornea sometimes quite difficult. Many cannot look in the necessary direction, or if they do turn their eye, it is only for a moment; they cannot hold it still more than a few moments. This first difficulty being overcome, and the point of the scalpel having traversed the cornea, it frequently happens that the most movable eye remains very quiet, and the rest of the operation is finished with the greatest ease. But this is not always the case, for some patients, the moment the point of the knife penetrates the globe of the eye, forcibly turn the organ inward, and it requires great skill to be able to follow this movement and finish the section without failing to hold the instrument properly.

2d. Another difficulty presents itself much more frequently, and often makes the operation much longer than it would otherwise be. It depends on the incision of the capsule. That the eye should be motionless during this period is still more necessary than during that spent in the division of the cornea, for the object we have in view is to carry through the pupil, which is always very narrow, an instrument which is at once pointed and cutting, and to open the capsule without injuring the iris or the vitreous humour. Now, during the whole of this time, there is absolutely nothing to keep the eye steady, because the least pressure upon it might be fatal. It is sometimes necessary to make five or six attempts before the

patient remains two moments, perfectly motionless, in a position by which the pupil is not wholly hidden by the eyelid.

No one could believe how many of such difficulties arise to perplex a surgeon who has not been accustomed to these delicate operations; but, finally, these are merely difficulties, and if they are overcome they cannot influence in the least the result of the operation; moreover, it is possible to avoid them almost entirely by gradually, by two or three days' exercise, accustoming the eye to remain motionless.

With respect to the accidents which may occur, I have seen the following:

1st. Once the section of the cornea was not sufficiently large to allow the crystalline to pass, and the opening was enlarged by means of a pair of scissors.

2d. It sometimes happened that the operation remained unfinished; sometimes because after the introduction of the scalpel into the eye, a quick motion of the patient removed it; sometimes because a similar course has made the point of the instrument traverse the iris or pupil into the posterior chamber, and in drawing it back the evacuation of the aqueous humour has caused an obstacle to the completion of the incision; sometimes, likewise, after the section of the cornea and opening of the capsule, the pressure exerted upon the eye to enable the lens to get out has caused the protrusion of a quantity of vitreous humour, and the cataract has remained.

3d. Quite frequently the lachrymal sac or the conjunctiva of the large angle of the eye is punctured at the moment that the section of the cornea is finished.

4th. The iris is injured at times, and the edge of the pupil torn when the knife enters or is drawn from the eye. Sometimes, likewise, this membrane is pressed against the blade of the instrument, and gets over the edge of it. If, under these circumstances, we cannot excite its contractions by gentle frictions upon the cornea, and we continue to press the scalpel onward, it necessarily happens that the lower portion of the iris is cut.

5th. Finally, if the pressure upon the globe of the eye is too long or too great, or if the cells of the vitreous humour are injured by the incision of the capsule; if the muscles contract spasmodically after the incision of the cornea, a portion of the vitreous humour is always protruded from the eye, before or more commonly after the lens.

The influence of these various accidents upon the results of the operation will be examined when we treat of the prognosis in this affection.

ARTICLE VI.

CONSEQUENCES OF THE OPERATION.

Besides the accidents which happen during the operation, there are others which come on afterwards, and too frequently destroy all hope of success. Some are local and have a manifest connection with the organ that is affected; others must be considered inter-current affections, which develope themselves during the time employed in perfect cicatrisation of the wound of the cornea, and do not seem to have any necessary relation to the operation.

Let us first examine whether the patients in whom the operation is successful,[1] do not have any kind of accident during the cicatrisation of the wound; and if any, what is their nature, and to what extent may they exist without danger?

I. SUCCESSFUL CASES.

Among the patients who underwent the operation of extraction of the lens in both eyes, in twenty-five the twofold operation succeeded; among those who were operated upon in one eye only, in twenty-six the operation was successful.[2]

[1] I have considered successful, not only those cases in which, after the operation, the pupil is perfectly clear, regular, movable; the cornea perfectly transparent throughout, and the vision so fully restored that the patients were able to see the most minute objects, but likewise those in which the sight, without being so perfect, is yet good enough to be of great service to the patients, and to allow of their walking without a guide, and easily recognising any object of which they were obliged to make use. In these cases the pupil may be more or less irregular; the iris may adhere in some points to the cicatrix of the cornea, or even form a small hernia through the lips of the wound; the borders of the incision may be opaque over a small surface, &c. In all these cases, I repeat, provided a space of sufficient size and clearness remains, that the luminous rays may reach the depths of the eye, vision may be performed to an extent that is very useful. These are my *successful* cases.

The cases of partial success are those in which the eyesight is re-established in too imperfect a manner for the patient to be able to walk easily alone; and likewise those in which the patients left the hospital before a perfect cure was obtained. Finally, the unsuccessful cases are those in which even slight advantages were not obtained.

[2] Among the cases of this double success is that of a man spoken of in page 356, *fifth case*, and, although he did not recover his eyesight, the two corneas and two pupils were perfectly clear, and there was nothing to prevent the transmission of light. It was the same with a woman, who died on the eleventh day in consequence of some disease not at all connected with the operation.

Among the cases of success in one eye was that of a man, who died on the fifth day, and of whom mention will be hereafter made; there was no trace of inflammation of the eye. A second patient, evidently affected with amaurosis; on leaving the hospital one could not have doubted that he had undergone an operation, as the cornea was clear, the pupil black and regular, but the eyesight was gone. Finally, a third, of whom mention is made, page 355, second case.

Now, out of fifty-one patients, there were five only who never experienced, at any period after the operation, any pain in the head or eyes. In all the rest the case was otherwise. They may be classed as follows :

1st. Some felt either for some few hours, or more generally for the first twelve, twenty-four, or even thirty-six hours after the operation, sensations of smarting, pricking, more or less severe pains in the eyes without headache ; when these pains were severe at the beginning, they subsided quite quickly, and after the first, or at the farthest the second, day they ceased wholly. This was the case in a quarter of those who were operated upon, (about thirteen patients.)

2d. Others experienced, during the same time, the same pains ; but with them a headache, which was in general limited to the forehead, on the side of it corresponding to the eye that had been operated upon, sometimes to the temple. This occurred in a third part of the patients, (eighteen.)

3d. Others, after having experienced, during very nearly the same time, the accidents of which I have just spoken, ceased to have any suffering for two, four, eight, and even twenty days ; and then the pains again came on, either in the head or eyes, and lasted, with different degrees of intensity, for one or several days without producing any unpleasant result upon the condition of the eye. (Eleven patients.)

4th. Finally, a few had pain in the eyes for many days after the operation, with or without headache, but generally these pains were slight.

Some other accidents happened, but more rarely ; for example—*toothache*. This pain, which is experienced sometimes after the operation for cataract, by extraction, is felt about the roots of the teeth of the upper jaw, on the side of the eye operated upon. Generally it is an unfavourable sign, but it does not seem dependent upon decayed teeth, for it is observed when the teeth are perfectly healthy, and in some cases where there are no teeth in the part referred to as the seat of pain. Finally, the frequent coincidence of this affection with pains in the forehead, temple, sometimes the cheek bone, and even the ala nasi of the same side, sufficiently indicate that they are owing to some affection of the fifth pair of nerves. The result of the operation in these cases is not necessarily unfortunate, for this pain was experienced in five of the fifty-one cases of those operated upon. The most remarkable case of this kind was that of a woman, fifty-three years of age, who, three years previously, had had an unsuccessful operation performed upon the left eye. In this woman the operation of extraction was very tedious ; the opening of the capsule very difficult, in consequence of the constant motion of the globe of the eye, and the iris was probably wounded, for a small quantity of blood flowed from the internal part of the eye ; the protrusion of the crystalline was followed by that of a quantity of vitreous humour, equal in size to a large pea. During the first twenty-four hours there was a pretty

severe pain in the eyebrow, cheek, and along the upper teeth of the right side of the face, but no cephalalgia, nor pain in the eye itself. During the succeeding twenty-four hours there was scarcely any pain at all. On the morning of the third day an erysipelas, without any precursory symptoms, appeared on the *left* cheek; the cheek, the left side of the nose, were red, swollen, not painful to pressure, hard to the touch; the skin of the patient was burning, dry, and there was intense thirst for twenty-four hours. On the morning of the fourth day the erysipelas was not sensibly altered in character; the next day, forty-eight hours after its commencement, it was almost wholly gone, only a slight hardness of the skin remained upon the ala nasi; the right side of the face perfectly healthy; there was no febrile excitement. The day on which the erysipelas began, a pretty severe cephalalgia commenced, confined to the right side of the forehead, accompanied by very painful pricking sensations in the eye; all of which ceased at the same time that the cutaneous affection did, but the pain in the eye began a day afterwards, and continued alone for four or five days. Afterwards, the patient was pretty well for ten or twelve days; only her eye was very sensible to light. Then a chill came on which was followed by heat and sweat. At the same time an intense headache reappeared, limited to the right side of the forehead, accompanied with very severe lancinating pains in the eye, cheek, and right side of the nose, the appearance of which parts was natural. The next day all the febrile action had ceased, but, until the moment of leaving the hospital, that is for ten days, the patient frequently experienced the same pains in the eye, temple, eyebrow, cheek, and nose, on the right side of the face. During all this time there was almost no redness of the conjunctiva; the cornea remained clear; the pupil was oblong, but very black. This woman could easily see a needle; the iris was connected with the incision of the cornea, entered it slightly but formed no hernia externally.

Besides the local symptoms, of which I have just spoken, some patients experienced others of a different nature. Thus, four of them had symptoms about the digestive organs.

1st. One patient, aged sixty-four years, in the enjoyment of good health, after having suffered during the day of the operation from very severe pains in the head and eyes, was seized in the evening with copious vomiting, after which he experienced relief, and passed a quiet night. From that period he had no inconvenience.

2d. In the three others there was little or no pain for eight, ten, or twenty days. On the tenth day one was taken with colic and diarrhœa, which lasted forty-eight hours, and had no unfavourable effect upon the eyes. The second had, on the eighth day, a violent chill, lasting three hours and not followed by heat or sweat. After this chill he had two turns of vomiting, and a copious diarrhœa, (at least twenty dejections during the night), without colic but with great thirst. The next day the skin was of natural temperature, the pulse was at 80, the tongue white and a little moist. Rice water with gum was administered. The diarrhœa continued three

days, the dejections were very numerous; there was no tenesmus, no smarting at the anus, no pains in the bowels, and I could not learn whether there was any blood in the discharges. There was no febrile action, nor the least pain in the head or eyes at that time nor afterwards. At the end of three days the diarrhœa ceased. In the third patient the symptoms, which came on the twentieth day, were very nearly the same as those I have just described. He was seized with very severe dysentery; bloody dejections every quarter of an hour, much tenesmus, severe colic, prostration. This condition lasted for three or four days, and yielded to the administration of some opiate enemata. The eyes did not suffer in the least.

In the four other patients there was a slight febrile excitement, without any appreciable cause.

It was limited in a man and woman to a little heat and sweat for the first three nights without any effect being produced upon the eyes.

Another woman experienced some uneasiness; her skin was hot and her pulse frequent, during the three days only after the operation. This state continued two days; there was no pain in the eyes, and merely a little sensation of weight in the head.

In a fourth, the febrile action did not come on until four days after the operation; the patient attributed it to exposure to air, from a window left open near her bed; she had a hot skin, uneasiness, great thirst, pain in the head and eyes during a whole night. Two days afterwards the same accident occurred one night accompanied with nausea but without vomiting; on the following day the patient was well, but in the evening the same circumstances occurred during some hours merely, and never afterwards.

A man, aged forty-six years, was taken, seven or eight days after the operation, with an eruption of very minute red points, which covered the chest and arms. This lasted four or five days without seeming to exercise the least influence upon the state of the eyes, which at its commencement were a little red, painful, and sensible to light. Moreover, this patient left the hospital with two hernias of the iris, having irregular but very black pupils, and the patient was able to see perfectly well.

Finally, the last patient was a man, fifty-five years of age, who was seized with symptoms which proved rapidly fatal. The day of the operation he experienced only a few lancinating pains in the eye that was operated upon, and a very slight pain in the head. The first night was tranquil notwithstanding a copious sweat that took place; the next day there was no pain; the tongue was natural; the pulse seventy-two. On the morning of the third day this man was confined forcibly to his bed; he had been delirious, during the night; he had even fallen upon the floor; at the visit he had become calm and was rational; he suffered no pain in the head nor in the eyes, but complained of uneasy sensations and of great thirst; his skin was hot and covered with sweat; his pulse 88, full and regular; tongue natural. In the evening delirium came on again; during the whole night the patient was talking and trying to get out of bed. On the morning of the fourth day he

was again rational; the skin was hot and moist; the pulse 100, respiration 40; face pale, tongue dry, red, and cracked; abdomen a little distended, soft, sensible to pressure, (venesection.) On the following night tolerable degree of calmness; the morning of the fifth day expression of extreme agony; breath panting; mind not disordered; face pale; pulse 120, small, soft; skin warm, respiration 48; auscultation performed in front merely afforded a sound of a large moist râle, mixed with a snoring sound; no difference between two sides on percussion; expectoration of white frothy purely mucous sputa. He died at 6, p. m. I was unable to attend the autopsy, but I learned that the eye that had been operated upon presented not the least traces of redness nor of any disorder whatsoever. All that was found, (at least so it was told to me), was considerable serous fluid in the cavity of the arachnoid.

It will be proper to mention, in this place, the case of a woman, aged fifty years, naturally feeble and very thin, both of whose eyes were operated upon, and upon whose case, unfortunately, I have only a very few details. This woman said that for two years before, she had experienced a sensation of heat in the region of the stomach; she drank much, and was constantly tormented with hunger; but she had never had vomiting, nor diarrhœa, nor cough. The day on which the operation was performed, she experienced some pricking sensations in the eyes, and a little headache. Her night was good. The next day the eyes were not painful; only a slight degree of headache remained; the tongue was natural; pulse 76. During nine following days, there was not the least suffering, no redness of the eyes; the eyesight was good, so as easily to distinguish the hands of a watch. On the tenth day, I caused the patient to rise and to sit up, in order that I might examine her eyes; she felt no pain, but she was rather feeble, and it was with difficulty that she was able to sit. Nothing peculiar happened during the day; the patient made no complaint. The next day she was found dead in her bed. The autopsy was made May 29, 1833, about thirty hours after death.

Neither of the eyes were, in the least, red; the two cicatrices of the corneæ were scarcely visible; the pupils were perfectly black; I did not think to examine whether the vitreous humour projected into the place of the crystalline lens.[1]

[1] The dura mater had nothing peculiar in its appearance; the great cavity of the arachnoid was moist, but had no effusion in it; the pia mater was slightly injected; under the arachnoid there was a considerable quantity of liquid. The lateral ventricles contained about an ounce of clear serous fluid; the white central parts, that is to say, the fornix and septum, were very much softened, and portions of it were easily detached, as whenever the parts were touched; the surfaces of the optic thalami, and corpora striata, were evidently softer than natural, but every where else, in the medullary mass, the gray and white substances had their natural colour and consistence.

The right cavities of the heart, of ordinary dimensions, contained some yellow clots of a semi-transparent and gelatinous character; the different orifices were not at all contracted. The parietes of the right ventricle were half a line thick; those of the left were from four to five lines.

The two lungs were not adherent; were of a gray fawn-colour, soft,

II.—UNSUCCESSFUL CASES.

Let us now examine the symptoms experienced by the patients in whom the operation was not successful, either from the fact that

spongy, light, crepitating throughout; contained not the slightest trace of tubercles; the bronchial tubes were pale.

The abdomen had no effusion into it. The stomach, which was rather large, contained a considerable quantity of the remains of unripe fruit, and orange seeds. The mucous membrane was mammillated in the cul de sac, and along the larger curve; its colour varied from gray to a pale rose colour, and in some points was of a vivid red, but it was every where of natural thickness; and strips, from three to four lines, could be raised from the great cul de sac; from six to eight along the great curve, and upon the two faces; and of more than an inch along the small curve.

The small intestine contained yellow mucous substances above, which became stercoraceous and soft below; its colour, internally, was every where of gray, tinged with red, except towards the upper part, where it was yellowish; a great number of venous arborisations, not very minute, were seen under the mucous membrane, which was very thin, and gave strips of at least six lines; the mesenteric glands were small. The large intestine was not examined.

The liver, of a deep brown colour, containing much blood, was, however, of its usual size and consistence.

The right kidney, at least two inches longer than the left, was about a third larger, but was of its usual form. Its fibrous membrane being transparent, was raised slightly by numerous points of a yellowish white colour, and united in groups over the whole surface of the organ. This coat being removed, the surface of the kidney appeared wrinkled, as it were, by this multitude of yellow points, which resembled, in appearance, miliary tubercles collected together in small groups. The organ was divided, lengthwise, into two equal portions; the same appearance was presented by the cut surface, but the yellow points, which were of the size of a pin's head, occupied merely the cortical substance, and not at all that of the tubes; this last seemed natural. By letting water fall upon this surface, and by pressing slightly with the fingers, it was easily perceived that the points were merely small collections of pus, for the liquid which constituted them being of a cream-like character, flowed wholly away during the time that the water was being poured upon it, and left a multitude of small cavities, having exactly the form and size of the yellow points. The ureter of this side presented nothing very remarkable in its aspect; the bladder contained a quantity of somewhat flaky urine; its mucous membrane was pale and thin. The wound was healthy.

This autopsy seemed to me to be sufficiently interesting to be given with some details, though they may not seem to have any but an indirect bearing upon the history of cataract. The absence of very appreciable lesions in the abdomen, save that of the kidneys, tends to make one think that it is to this last lesion that we must refer the symptoms experienced by the patient, in the neighbourhood of the stomach, for two years before death; it is to this, likewise, we should refer the extreme emaciation. It is very much to be regretted that she was not examined upon this point more minutely. An inflammation of the cortical substance of the kidney terminating by the formation of a multitude of small purulent cavities is, I think, a disease but little known.

With regard to the proximate cause of death, the extreme debility of this woman on the day before she died might lead us to believe that the softening of the central parts of the brain was, in fact, a pathological effect, and not cadaveric softening. However, we must not forget that about thirty hours had elapsed from the time of death, that the weather was warm, and that there was a little serous fluid in the ventricles.

the two eyes operated upon were lost, or that one was lost and the other saved; or, finally, from the operation having been performed on one eye only, and without success. The following are the results:—

Out of sixty-one patients affected with cataract, and who were in the circumstances mentioned, fourteen presented, at some time after the operation, considerable febrile action, especially during the night. Eleven had the toothache mentioned above, always on the side of the eye most affected. In four of the eleven patients, the teeth were healthy; in a fifth, the pain was situated in the gums, for there was not a single tooth on that side of the head; in the three others, the parts affected contained carious teeth. In two patients, vomiting came on; in a third, nausea merely. Some had a little diarrhœa. A very aged woman, of whom mention will be made hereafter, was seized with erysipelas of the face, which destroyed her in less than forty-eight hours.

With regard to the symptoms which have immediate reference to the operation, namely, pain in the eyes and head, we have seen that they have been observed even in patients who recovered their eyesight. Among those who did not have this good fortune, these symptons had, commonly, a greater degree of intensity, and longer duration. This is very nearly the only difference which, in this respect, the two classes of patients presented; and we should err, should we believe that this is always so, and that the mildness of the pain, or even its total absence, is a sure proof of a successful termination. In fact, out of these sixty-one patients, there are ten whose eyesight was wholly destroyed, either by opacity of the cornea, or by the formation of a membranous cataract, and obliteration of the pupil, almost without any pain in the affected side. In one, there were at the same time obliteration of the pupil of one side, and capsular cataract of the other; nevertheless, the patient scarcely felt, from time to time, slight pricking sensations in the eyes, and a little heaviness of the head, during the first days. In three others, who likewise lost both eyes, there was pain in one eye only, and none at all in the other. The cornea became opaque in one, and capsular cataracts took place in the others. Similar circumstances occurred in the last six.

It frequently happened, as in the most favourable cases, that the pain disappeared on the second or third day to appear again afterwards. Sometimes, likewise, it was not until some days after the operation, that it came on for the first time, and it was speedily followed by the loss of the eye at the very moment it seemed out of danger. The most remarkable case of this nature, was that of a country labourer, aged fifty-six years, who had been operated upon for cataract of both eyes. He experienced some slight pains in the eye, in the forehead, and ala nasi of the right side; there was a little redness of the conjunctiva; the patient saw, but not very distinctly. From the first day, there was not the least pain in the left eye, there was no redness of it; the eyesight was excellent, the patient being able to distinguish the hands of his watch. On the

evening of the eleventh day, when making some efforts to go to stool, he was seized with great pricking sensations in this eye. During the night, these prickings were very much augmented; they then consisted in very severe pulsations, which were accompanied with very great pain in the left side of the forehead, and left lobe of the nose; numbness about corresponding teeth; the conjunctiva was swollen; the upper eyelid could not be opened, and formed a projecting tumour of the size of a nut; a sero-purulent fluid was escaping from that eye, which, after the lapse of a few days, was wholly destroyed by suppuration.

The woman who died of erysipelas of the face, was aged eighty years, and both eyes had been operated upon; from that moment, she never ceased having severe throbbing in the left eye, and forehead, at the same side. The fifth day the cornea was already opaque; the conjunctiva began to be swelled. On the tenth day, the eye, or rather the upper eyelid, made a prominence of the size of a large nut; the skin was of a reddish colour. Granulations of a bright red colour began to protrude themselves from between the lids; in the place of the cornea, there was a white opaque patch, and there was a sero-purulent discharge.

Up to this period, there was almost no pain in the right eye; the conjunctiva was scarcely reddened, nevertheless the pupil was already occupied by an opaque body. On the fifteenth day, in the morning, the patient was reported to have been as follows, from the previous day:—Right side of the face swollen, hard to the touch, of a slightly livid red colour; some phlyctænæ near the mouth; skin warm, dry; pulse 120, regular; tolerably strong; respiration 36; somewhat noisy; marked somnolency; patient mutters some unintelligible words; can scarcely show her tongue, which is yellowish, dry, cracked. She expired at 9 P. M.

Autopsy, May 31, 1833, thirty-seven hours after death.

Face.—The redness of the erysipelas had disappeared, and there was no longer any tumefaction; the sole remaining traces of the disease were some brown dried crusts, and a great fragility of the epidermis, which was removed by the slightest contact, especially about the right upper eyelid. The conjunctivæ were of a very pale rose-colour, and there was only a very slight degree of swelling on that of the left. Both eyes were removed from their orbits; the muscles and the fat surrounding them presented no appearance of lesion. The *right* eye was of its usual size; the cornea was clear; behind it there was a yellowish patch, occupying the field of the pupil. This patch was a false membrane, very thin and very soft, resembling those formed on blisters, and it was adherent to the whole of the small circumference of the iris. After having separated it, it was easy to see a second opaque pellicle perfectly distinct from the former, and behind it. It was the anterior lamina of the capsule; otherwise the whole of the eye, the iris, vitreous humour, and retina were natural. The *left* eye was evidently larger than usual. A yellow, uneven and soft patch occupied the place of the cornea, and extended into the interior of the eye. We

could with difficulty recognise some blackish remains of the iris; the vitreous humour was changed into a yellowish, homogeneous, opaque mass, having the consistence of soft cheese. It was evidently semi-concrete pus. This mass was surrounded by a brownish membrane, thin and somewhat rough, formed by the union of the choroid coat and the retina, which could not be distinguished from one another.[1]

This case is another example of the formation of a capsular cataract, and even of a false membrane, without any appreciable pain having been experienced by the patient, either in the eye or in the corresponding side of the head. The fact of the existence of a false membrane all around the pupillar edge of the iris and not existing elsewhere, seems to me to be quite interesting. It is a point in pathological anatomy which has been as yet but little studied, although something analogous is found in a memoir by M. Boudant, to be found at the end of the account of the doings of the Anatomical Society during 1829.

Nothing is more rare, whatever may be said to the contrary, than to see an adult die of erysipelas of the face, at least when this disease occurs in one who has not been debilitated by previous disease, and is otherwise healthy. A certain degree of somnolency and even of delirium are, however, rather frequent in this exanthem, and it is probably this symptom which has made so many physicians fear the supervention of meningitis. Doubtless, it is when death occurs amid cerebral symptoms that we ought, if at any time, to find a confirmation of these fears; nevertheless we have seen that the membranes were perfectly healthy.

In recalling what has been said with reference to the symptoms occurring after the operation for cataract, we must, I think, conclude that we ought not to rely upon the slightness of the pains,

[1] *Cranium.*—The large cavity of the arachnoid contained two small spoonfuls of reddish serous fluid; a somewhat copious infiltration of the same fluid was observed between the arachnoid and brain; in no point did we discover the least traces of pus or of thickening of the meninges; the large veins of the pia mater were moderately injected, and were easily raised without tearing the cortical substance of the brain. This last, and likewise the medullary substance of the brain, were of natural colour and consistence. I should, however, except the central medullary portion, and especially the fornix; the two laminæ, forming its anterior pillar, were so softened that they remained adhering to the corpora striata when we attempted to raise the corpus callosum. Their consistence, however, was not at all pulpy. The lateral ventricles contained one or two small spoonfuls of clear serous fluid. The plexus, of a pale rose colour, contained many transparent vesicles of the size of small peas, on the outside of which were numerous very minute arborisations. The cerebellum and annular protuberance presented no marks of disease.

The remainder of the autopsy was made with many details, but I think it unnecessary to repeat them here. I would simply state that there was a slight general emphysema in the lungs, that the heart and its orifices were healthy, that the mucous membrane of the stomach was reduced to about half its usual thickness, but was not evidently softened; and that in respect to the small and large intestines, there was a slight degree of softening about the ascending colon.

or even their entire absence, for a diagnosis that the operation will succeed, any more than we should despair of success when these pains are observed, especially if they do not last more than twelve or twenty-four hours. In general, we may say that they are more severe, and more constant in those cases in which the eye is destined to be lost than in the opposite ones, but how can we trust such differences as these, when we would make prognosis or oppose symptoms which may destroy the sight? Toothache, it is true, occurred twice as often in unfavourable as in favourable cases, but the uncertain time of its commencement, which is frequently very late after the operation, takes away much from the value of this symptom, since it announces evil when it is already too late to remedy it. Finally, we must make a careful examination of the eye itself if we wish to know at their commencement the different lesions which we fear, and happy shall we be if art possesses the means of well-proved utility in preventing or curing them.

The treatment made use of against the accidents occurring after an operation is generally limited to mustard pediluvia several times a day, and very seldom is any recourse had to local or general bleeding, or to purgatives.

ARTICLE VII.

RESULTS.

Before passing to a very important question, viz., the prognosis, it seems proper to make known the results which have been obtained, in order that we may appreciate more easily the value of the different circumstances which may influence these results.

When engaged in researches of this nature we ought to distinguish (as it was remarked by M. Roux,) that which relates to the eyes from that which has reference to the individual; where the chances are always more favourable for the latter than the former; that is to say, there are more persons operated upon who recover sight, than eyes upon which the operation is successful. The proportion of successful and unsuccessful cases in this twofold point of view is as follows:

One hundred and fifteen subjects underwent the operation of extraction of the cataract, either from one or both eyes. Of these one hundred and fifteen, seventy-three owed to the extraction of the lens the recovery of sight, which gives $\frac{73}{115}$, or a little *more* than five in eight, for the proportion of persons upon whom the operation succeeded out of the whole number of those who were operated upon.[1]

[1] The difference between $\frac{75}{115}$ and $\frac{5}{8}$ is equal to $\frac{1}{1040}$ nearly.

Taking into view the eyes only, we find one hundred and seventy-nine of them upon which extraction was performed. Of these one hundred and seventy-nine operations, ninety-seven were successful; that is to say, a little *less* than five in nine was the proportion of successful cases to the whole number of operations.[1]

On the other hand, the following are the different kinds of lesions which destroyed the eyesight in the unsuccessful cases.

1st. Fourteen eyes were destroyed by suppuration.

In these cases, at first, a very severe inflammation commenced in the conjunctiva in all its parts. This membrane swells considerably, and forms a large prominence of a vermilion-red colour, which pushes forward the upper eyelid and causes it to project sometimes to the size of a nut, or even of a small apple, owing in part to an œdematous infiltration. It secretes, at first, a thin purulent fluid, afterwards true pus; the interior of the eye is afterwards seized upon by the suppuration, which terminates only with entire evacuation of the eye. It is after such accidents as this, that we find the organ reduced to a mere tubercle, more or less irregular, and it is possible then to correct the deformity by means of an enamel eye, the appearance and motions of which imitate very perfectly those of the true eye.

We must not confound with this inflammatory swelling of the conjunctiva, another lesion somewhat like it, but which is of a much less grave nature. I refer to a kind of slight chemosis of a livid, red colour, very brilliant, of a semi-transparent aspect; rough, and which is seen sometimes to form in twenty-four hours. It does not suppurate, and exists more generally, I think, in the lower than the upper eyelid. Although it may not be uncommon to see it coincide with the formation of a capsular cataract on total or partial opacity of the cornea, I have seen it develope itself upon eyes which have not been thereby lost.

2d. Nineteen cases of opacity of the cornea so perfect as to prevent any thing behind it from being seen.

3d. Nine cases of incomplete opacity of the cornea, leaving an opaque spot to be seen behind, in the field of the pupil.

4th. Fifteen cases of very evident membranous cataract without opacity of the cornea, and without my finding indicated in my notes any marked change of form, or displacement of the pupil. In three or four cases, these cataracts presented a very evident uniform rosy tint.

5th. In six cases there was an opaque body in the field of the pupil, with displacement or very marked malformation of this opening.

6th. Once only, complete closure of the pupil.

7th. There remain eighteen cases which could not be introduced into the preceding divisions. One was that of a man in whom vision was destroyed, though the pupils were both very black. A second, who presented no opacity save in a very small part of the

[1] The difference between $\frac{97}{179}$ and $\frac{5}{9}$ is equal to $\frac{1}{715}$ nearly.

pupil, yet he could see nothing. Two very curious cases in which, during the time of cicatrisation of the incision, a body of an irregular shape, of an opaline white colour, resembling very much a deformed cataract during the extraction, interposed itself between the lips of the wound of the cornea. It adhered strongly although it was large enough to raise the flap, evidently. In one of the individuals the body seemed to come out daily, more and more, and after having a long time raised the flap, it at length protruded wholly out of the eye, but it remained firmly adherent to the exterior of the cicatrix. It was of an opaline colour, hard to the touch, and insensible. In these two patients the extraction was perfectly performed, and after it there was no appearance of opacity remaining in the field of vision. Was it a kind of abnormal vegetation, a deviation of the fibrous tissue of the cicatrix? Whatever may be the fact, eyesight was destroyed in one of these patients by the formation of a capsular cataract, at the same time that the excrescence appeared; in the second, the pupil seemed very nearly quite well, but vision was very imperfect.

Many of the other patients were among those whose cases were called those of partial success, (*demi-succès,*) either because they left the hospital with too great sensibility about the eye for us to be able properly to examine them, or because they had a partial opacity of the cornea and pupil. Finally, in some cases, the lesion was not sufficiently described for me to be able to indicate it now; this omission occurred especially in relation to the iris. I find mention made in three or four cases merely of any change of colour, and of a duller hue that this membrane presented, but I would not assert that cases of iritis may not have been more frequent.

In conclusion, with reference to these different lesions, we see that membranous cataracts existed evidently in more than one third of the unsuccessful cases. It seems, at the first glance, as if this were in direct opposition to what I previously asserted in relation to the infrequency of this kind of cataract, but the contradiction is only apparent. In fact, I have spoken of cataract in general, and not of the patients who leave La Charité after having undergone the operation without success. Doubtless, we may seek in M. Roux's mode of operating for the cause of so many capsular cataracts, which are irreparable accidents. Is it not evident that by remaining satisfied with a simple incision into the anterior part of the capsule, this membrane may become opaque, since it remains in the eye, while it would be impossible for such a thing to happen if it were cut into many fragments according to the advice of Beer, and drawn out from the eye, at least in part, at the same time with the crystalline lens? These patients are very peculiarly situated, as we have seen, and to disregard this fact, and draw from them general conclusions with respect to the frequency of membranous cataract, would be as illogical as to say that extraction is fatal once in fourteen times, merely because out of forty-three individuals operated upon for cataract, during the spring of 1833, there were three whose constitutions were in such morbid condition that they

died. Moreover, the facts related in another part in reference to this question seem to me to be unanswerable proofs.

ARTICLE VIII.

PROGNOSIS.

These general results being pointed out, let us now examine some facts with reference to prognosis, and notice, successively, the influence of age, sex, single or double operation, and some of the symptoms which complicate it.

I. *Age.*—In regard to age, the patients may be divided into three series.

The 1st contains 44 individuals aged from 20 to 59 years.
2d " 40 " 60 to 69 "
3d " 27 " 70 to 82 "

The males and females being about equal in both series.

In the first series sixty-nine operations were performed, of which thirty-nine succeeded, and twenty-eight patients recovered their eyesight. This gives for the patients the proportion of 28 to 44, very nearly equal to the proportion of 5 to 8, and for the eyes the proportion of 39 to 69, which differs very little from the general proportion of 5 to 9.

In the second series sixty-four operations were performed, thirty-four successfully; twenty-seven patients recovered their eyesight. This gives for the patients the proportion of 27 to 40, a little more favourable than 5 to 8; and for the eyes the proportion of 34 to 64, rather less favourable than 5 to 9.

Finally, in the third series, forty-four operations were performed; nineteen successfully, and fourteen of the twenty-seven affected with cataract recovered their eyesight. In this, the proportion of 14 to 27 is less favourable than that of 5 to 8; and for the eyes, that of 19 to 44 is still more unfavourable.

Thus, we see that age between the periods of twenty and sixty-nine years, does not seem to cause any appreciable difference in the proportion of successful cases, whilst from seventy to eighty-two the results are evidently less favourable, whether we consider separately the eyes or the individuals; and moreover the loss of a portion of the vitreous humour, the unfortunate influence of which accident is incontestable, does not happen oftener in this series than in the other two.[1]

[1] There is another circumstance which has not been mentioned, and to which we must attribute the greater proportion of unsuccessful cases at a very advanced age: I refer to the withering of the cornea. In a few patients, especially old persons, this membrane remains wrinkled or even concave anteriorly, after the extraction of the lens, instead of retaking its

II. *Sex.*—As to the influence of sex, we have the information afforded by the following facts. Of one hundred and fifteen patients operated upon, fifty-nine were men and fifty-six women, and upon these one hundred and fifteen, one hundred and seventy-nine operations were performed, ninety upon men, eighty-nine upon women:

73 patients recovered their sight,	40 men, 33 women.[1]
97 operations were successful,	52 upon men, 45 upon women.[2]
72 were unsuccessful,	32 upon men, 40 upon women.
10 were partially successful,	6 upon men, 4 upon women.

Thus, in every respect, the results of the operation were less favourable in women than in men. We have already seen, in studying the course of cataract, that the former are, under every point of view, greater sufferers in this disease than men.

Reflecting upon the different circumstances in which the men and women are placed, who are operated upon for cataract at La Charité, I at first thought that the greater want of success in the latter might be, in some degree, owing to their being obliged, when going to bed, to ascend a staircase which communicates with the external air; but I acknowledge that I now very much doubt the reality of this cause, since I have seen in a memoir upon cataract, published by M. Lusardi, that the number of his successful cases has been a little less in women than in men. In fact, it is clear that here there is nothing similar to what occurs at La Charité, in consequence of the arrangement of the wards.

convex shape, as it is usual for it to do. This is a very serious accident, as may be easily conceived, since it interferes with the exact meeting of the edges of the incision, an indispensable circumstance for the immediate reunion, and an essential condition for the success of the operation. Unfortunately, I cannot give more positive details upon this point, as my notes do not contain those which are sufficiently precise. In one case of this description, my uncle, Professor Maunoir, directed his patient to lie horizontally, and filled the concavity, which is formed by the projection of the circumference of the orbit, with warm distilled water; then opening the lids very slightly, he saw the contraction soon disappear by the introduction of the water, which thus replaced the aqueous humour. The patient, who was a very old man, was treated as usual, and perfectly cured. Those surgeons, who might apprehend dangerous consequences from the presence of water in the interior of the eye, must remember that it scarcely differs from the aqueous humour; and, what is still more encouraging, that the experiment has been tried with success a number of times. The oculist, Forlenze, who was considered very fortunate in his operations, adopted as a general method for removing the fragments of the lens, injections of warm water into the interior of the eye.

[1] To have the success proportionally equal, there should be thirty-eight women instead of thirty-three.

[2] For the above reason there should be fifty-one women instead of forty-five.

III. *Single or double operation.*—A question of great importance, and one which is often discussed, is this: Are there more chances of success for each eye, when we operate upon both of them the same day, or when we do not perform the operation upon the second eye until after the first is cured? On this point, as on all others, opinions are divided. M. Demours maintains that we must make a distinction in this question. In his opinion, the double operation, performed the same day, is more advantageous to the surgeon, because it is rarely the case that both eyes are lost, and it is sufficient to save the reputation of the operator for the patient to recover the sight of one eye; but he thinks that the patient's chance is more favourable, if the operation on the second eye is performed after the first is cured; the latter opinion only, as we see, is a scientific one. Professor Dupuytren is of the same opinion. Dr. Sanson, in an article on Cataract, in the Dictionary of Practical Medicine and Surgery, thus expresses himself:—"The practice of M. Dupuytren has *established without a doubt*, that when we operate upon both eyes the same day, the inflammation is more violent than when we operate upon one alone, and that almost always one of the two, taking upon itself this inflammation, is lost, and in some measure saves the other at its own cost; whilst we might more easily save both of them by successive operations." If the fact be as M. Sanson asserts, it must be acknowledged that the loss of one eye to save another, would be a revulsion dearly purchased; but such is not the opinion of M. Roux; this professor, on the contrary, thinks that by operating upon both eyes the same day, there is a little more chance of saving both, than if we did not operate upon the second until after the cure of the first.

In presence of these imposing authorities, let us see what facts declare. Various circumstances, which it is useless to state, induced M. Roux, notwithstanding his decided preference for the double operation on the same day, to operate upon his patients on one eye alone; this occurred in forty-eight of the patients whose cases I have collected. Now, out of these forty-eight operations, twenty-three were completely successful; we ought, I think, to add to these twenty-three, one case of amaurosis, which evidently existed before the operation, and of which the result, as an operation, was as successful as possible; we might also properly add the case of that man who died on the fifth day, and whose eye did not present any trace of inflammation; this would make the successful cases a little more than one half. But, to be more rigid, let us subtract these two cases from the sum total, and, of forty-six operations, we shall have twenty-three successful cases, twenty unsuccessful, and three partially successful. Now, in sixty-four other patients, extraction was performed on both sides the same day. Seventy-four of these one hundred and twenty-eight operations were entirely successful, forty-seven were completely unsuccessful, and seven were only partially successful. We have, then, definitely, seventy-four out of one hundred and twenty-eight for the proportional number of successful cases; that is, a result a little more favourable than in the

preceding series. Thus the dangerous influence of the double operation performed on the same day is far from "*being established without a doubt.*"

IV. *Accidents of the Operation.*—We have seen above that the extraction of cataract is complicated with certain accidents. I now proceed to examine their influence upon the final results.

The first, and, without doubt, the most trifling, is *the pricking* of the skin, or of the mucous membrane of the internal angle of the eyelids. I should not even have spoken of it, if I had not heard many surgeons say that this lesion, though trifling, being in the immediate neighbourhood of an organ so delicate as the eye, which has just undergone an operation, may be greatly inconvenient by producing a state of inflammation, or aggravating it, if it be developed. This opinion is, in truth, very rational; but on this point, as it often happens, experience does not at all confirm the decision of reasoning. This is proved by the following fact: I believe that I have never, or almost never, omitted noting this accident when it occurred; now, of twenty-seven operations, in which I find it stated, sixteen resulted favourably; which gives the proportion of sixteen to twenty-seven, a little better than the general proportion of five to nine.

A second accident, which sometimes occurs during the section of the cornea, is *the lesion of the iris.* This membrane is usually cut in a portion of its breadth, either externally, when the knife penetrates the eye, or internally, when it is withdrawn; sometimes at the lower part by the middle of the edge of the instrument. The result is a pupil more or less hollowed on the side of the lesion, and this hollowness most frequently continues after the cure. The wounding of the iris, considered only as a slight deformity, is evidently an imperfection in the operation; but is the lesion of so vascular and so sensible an organ a real inconvenience, and is its influence unfavourable upon the first result? In other words, are there more eyes lost when this accident occurs, than when it is avoided? Authors who have written upon the causes which prevent the success of the operation of cataract, have not failed to assign an important part to the lesion of the iris. Ware distinguishes it as dangerous. M. Sanson, in the article already quoted, thus expresses himself—"The blood which escapes after the iris is wounded, soon fills the anterior chamber of the eye, and obscures the parts to such a degree, that it is only by chance that we manage the instruments to complete the operation; and supposing that this is completed, there *always* results, from the lesion of the iris, an inflammation of greater or less severity, the consequences of which are either a deformity of the pupil, or secondary opacities, which injure the vision, or which altogether prevent it." I know not how I can, in any respect, adopt the opinion of Ware and Sanson on this point. I have observed the lesion of the iris twenty-one times; and of twenty-one operations, thirteen were successful, which gives

the proportion of thirteen to twenty-one, which is even more favourable than the general one of five to nine.[1]

As to the effusion of blood into the anterior chamber, it was not in any case sufficient to prevent the completion of the operation; still more, there was a certain number of patients in whom there did not flow a single drop of blood from the edges of the incision. This fact, however, is well known; we know that in some circumstances apparently similar, and without our being able to account for the difference, the section of the iris causes an effusion of blood, or is wholly free from it. It would be curious to examine which of these two cases is the most frequent, and I regret that I have generally limited myself to noting the lesion of the iris, without stating whether there was or was not an effusion of blood. I can, however, assert, that of eight cases, in which I did not make this omission, there were three in which not a drop of blood was effused into the eye; one in which there was a very small quantity only; four in which it was greater, but did not interfere with the operation.

As much may be said of the pain; it is sometimes very acute at the moment when the knife penetrates the iris; at other times the patients do not appear to feel any.

With respect to the final deformity of the pupil after the cure, it is true that it has occurred more frequently, but without being followed by observable difficulty in the power of vision, and without its being, besides, a result of inflammation, the effect of which would rather tend to remedy it. Moreover, this slight deformity, though frequent, does not always continue; and the iris, when it has been divided, presents a tendency to reunion which has appeared so great to Sir W. Adams, that this oculist, to obtain the continuance of the artificial pupil when we operate by the incision of the iris, recommends us to interpose a fragment of the lens between the lips of the wound, to prevent their union. It must, however, be allowed, that this tendency to reunion is not very great, at least for a certain time; for of thirteen patients, in whom the lesion of the iris occurred, and in whom I have accurately noted the state of the pupil upon their leaving the hospital, there were found eleven in whom this opening remained irregular, and two only in whom it had resumed its rounded form.

[1] If those authors who have pointed out the lesion of the iris as dangerous had taken care to count all the facts which they have witnessed of this kind, I think that they would have arrived at different conclusions. They probably trusted to their memory, which has reminded them of their unsuccessful cases only. I know of no more fruitful source of error. The small number of those who have taken the trouble to collect, without distinction and without choice, many cases of a disease, and who have afterwards scrupulously analysed facts to deduce the consequences which they contain, must have remarked that, upon a large number of points, the rigorous result of their observations was very different from the opinions which they had formed according to their recollection. And yet it must be acknowledged, that it is this opinion only which too often constitutes what we call the experience of a practitioner.

But if the lesion of the iris did not appear to have a more unfavourable influence upon the final results, than the simple pricking of the skin of the larger angle of the eye, the same cannot be said of *the escape of a part of the vitreous humour ;* and, what is remarkable, authors, though they mention this accident, do not appear, by their expressions, to attach any great importance to it, unless this humour has been evacuated in considerable quantity. According to Beer, when an eighth or even a fourth of the vitreous humour has escaped, the sight is but little, or not at all, altered; if a third of this liquid be lost, vision must be quite imperfect; and, finally, when more than half has escaped, the result is still less satisfactory. Professor Roux also thinks, if I mistake not, that the loss of a small quantity of the vitreous body, especially if the eyes be large, is not an unfavourable event. I believe that, even adopting this view, we ought to avoid, with the greatest care, whatever might occasion the accident in question. Who, in fact, can say, that if little or much of the vitreous humour escape once, that its cells are broken?

However, in examining a certain number of cases, we arrive at the conviction that this accident is serious. I have observed it nineteen times; and of nineteen operations, in which it occurred, six only were successful; whilst, subtracting them from the sum total, there remain one hundred and sixty operations, of which ninety-two were successful, which makes a little more than five successful cases out of nine operations. The difference, we see, is very remarkable; for we cannot suppose that, in all the unsuccessful cases, one half, one third, or even one fourth, of the vitreous humour was evacuated. I do not believe that I have once seen a quantity sufficient to be estimated as a third, and it was generally much less.[1]

I will recapitulate in a few words the principal results of this essay.

Causes.—*Sex* has not appeared to exercise an influence upon the production of cataract, in the patients who have come under my observation. Of one hundred and twenty-one patients, sixty-one belonged to the male sex, sixty to the female.

The influence of *age*, on the contrary, is indisputable: about a fourth of the patients with cataract was between fifty and sixty years of age; nearly a third between sixty and seventy; a little less than a fourth above seventy.

Hereditary influence must be considered as a real cause of cata-

[1] Since this work was formerly published, a volume of the new edition of the Dictionary of Medicine has appeared, which contains the article *Cataract.* The author, a young surgeon of great merit, speaking of the evacuation of the vitreous humour, says that he cannot admit my opinion upon the danger of this accident, because he has often seen that it does not injure the recovery of the sight. If I had written according to my memory, it is probable that I should have used the same language that he has, for the six successful cases which I witnessed have made much more impression on my mind than others. This point involves a question of method, and I regret that M. Bérard has not understood it.

ract, since it has been proved in at least a fourth of the patients who have been questioned on this point.

As to the most of the other circumstances which have been studied—as the profession of the patients, their mode of life, their constitution, the influence of certain diseases, &c.—I have explained the reasons which, in my opinion, prevent the actual appreciation of the value of these circumstances.

Commencement.—Cataract commenced rapidly in ten cases; slowly, on the contrary, in fifty-two.

It first affected one eye sixty-three times in seventy-two cases, (thirty-five times the right eye, twenty-eight times the left); in the last nine cases, it commenced at the same time in both eyes.

In eight of these seventy-two patients, the formation of the first cataract was entirely unperceived; chance only led to their discovering that they had lost the sight of one eye.

Symptoms and Diagnosis.—The sensation of a uniform mist was a constant symptom in all the patients affected with cataract.

In about three fourths of the cases (forty-six times in fifty-nine), there were some other sensations of the vision connected with this, the most frequent consisting in a black fly which the patients thought that they perceived.

Fifteen out of forty-six patients saw better at the side than in front; eight did not see better in whatever position they placed objects; twenty-three presented nothing remarkable in this respect.

In the great majority of cases, or forty-five times out of fifty-five, the patients saw better in a dark place than in a very bright light. The ten others had not observed a difference, or ever said that they saw better in full day; but in some of the latter there is a doubt as to the simplicity of the cataract.

The pupil was movable and regular, and the faculty of vision not entirely lost, in nearly all the patients; there were, however, some exceptions in some cases in which we could not suspect amaurosis. The appearance of the cataract was very various. The following was the order of frequency of the principal tints observed in a hundred and seventeen cases—*gray*, *white*, *bluish*, *blackish brown*. Besides the variety of colours, twenty-six patients out of sixty-five had cataracts of a spotted or striated aspect.

Capsular cataracts are far from being frequent; there were but five of this kind found in a hundred and seventy-nine patients who were operated upon by extraction.

The characters pointed out by Beer and Travers for recognising capsular cataracts are fallacious; these characters did not exist in the five cases mentioned, and were met with in a great number of cases in which the cataract was purely crystalline.

Duration.—The duration of the formation of two cataracts was as a mean, in forty-seven patients—a subtraction being made for sex—five years, one month, four days. For twenty-five men, it was four years, two months, twenty-three days; for twenty women, four years, five months, twenty days. This mean duration was, for the first eye affected, one year, seven months, eight days, in

men—two years, seven days, in women; and for the second eye, one year, four months and a half, in men—two years, fourteen days, in women.

The power of vision of the second eye, one being affected with cataract, continued more frequently, and for a longer time, in men than women.

Results of the Operation.—The results of the operation for cataract, by extraction, have been more favourable to individuals than to the eyes operated upon. Of a hundred and fifteen patients, seventy-three recovered their sight, which makes a little more than five in eight. Of a hundred and seventy-nine operations, ninety-seven only succeeded—that is a little less than five in nine.

In more than a third of the cases in which the operation was not successful (thirty cases), the loss of sight was owing to a secondary cataract, with or without partial opacity of the cornea, and with or without deformity and displacement of the pupil. In fourteen cases, the eye was destroyed by suppuration; nineteen times there was complete opacity of the cornea—once only closing of the pupil. In the last eighteen cases the lesions were various.

Prognosis.—The age of patients did not appear to have a sensible influence upon the result of the operation, from twenty to sixty-nine years; but, above seventy years, the results were a little less favourable.

The results were a little less favourable in women than in men, whether we consider only the eyes operated upon, or the individuals upon whom the operation was performed.

The results obtained by operating upon a single eye were less advantageous than those obtained by operating upon both eyes on the same day. This is the result of the comparison of forty-eight single operations with sixty-four double ones.

The pricking of the internal angle of the eye, which was observed twenty-seven times, had no unfavourable influence upon the result of the operation: of these twenty-seven cases, sixteen were successful.

The lesion of the iris did not produce more serious consequences, since, out of twenty-one operations in which this accident occurred, eight times only the sight was not recovered. There was not in any case a sufficient effusion of blood to prevent the completion of the operation; sometimes not even a single drop was effused.

The escape of a portion of the vitreous humour constitutes, on the contrary, one of the most unfavourable accidents in the operation for cataract, since six only of nineteen cases in which it was observed were successful.

AN ESSAY

ON

PYREXIA, OR SYMPTOMATIC FEVER,

AS ILLUSTRATIVE OF THE

NATURE OF FEVER IN GENERAL.[1]

BY HENRY CLUTTERBUCK, M. D.

INTRODUCTION.

Several years have now elapsed since I submitted to the consideration of the profession, the first part of an "Enquiry into the Seat and Nature of Fever;" in which I endeavoured to show, from the whole history of the disease, that fever, strictly so called, or what is termed idiopathic fever, is, in its origin, a local and not a general disease, or disease of the whole system; and that it consists essentially in inflammation of the cerebral substance, while the febrile symptoms attending it, namely, heat of skin, frequency of pulse, foul tongue, and others, are symptoms common to it and other inflammations. This doctrine, as might have been expected, has been variously estimated. By not a few, it has been adopted with little reservation, and has had a corresponding influence on practice. The change, indeed, that has taken place in the general management of fever, within the period mentioned, is very striking. Practitioners, with few exceptions, no longer entertain a dread of debility, as being the essential part of the disease, but consider this rather as the consequence of a previous state of vascular excitement—in the brain more especially; and they endeavour to prevent it, therefore, by anti-stimulant treatment at the outset of the disease. Few believe, at present, that a patient can be bled or purged into a typhoid state. On the contrary, it seems to be pretty generally understood, that such a state is more likely to be prevented, than induced, by an antiphlogistic mode of cure, provided it be promptly and actively applied; with the further advantage of cutting short, in numerous instances, a disease, which, however mildly it commences its career, is apt to run a protracted course with daily-increasing danger to the patient. According to my experience, instances now rarely occur (except from neglect or mismanagement in the beginning) of fever running out to a term of three or four weeks' duration, as was frequently the case formerly; and ending, at last, in a total destruction of the powers of life, principally through the injury done to the organ of the sensorial functions, the brain, which always appears to suffer in proportion to the violence and duration of the disease. In fact, the practice now most approved, and most generally adopted, in the treatment of fever, is, in all essential points, the same as that employed in the cure of inflammation in

[1] 8vo. pp. 136. London, 1837.

general; subject only to those qualifications and restrictions that naturally arise out of the peculiar nature of the organ primarily and essentially affected—a circumstance that serves to modify the treatment of all other diseases.

In the former volume, which contains the first part of the proposed "Enquiry," I laid down the general doctrine of fever according to the principle here stated. The second part will contain the application of the doctrine to the different varieties of the disease. The publication of this has been delayed much beyond my original intention; partly on account of other professional employment, but chiefly in order that time might be given for further observation; and, also, that I might be enabled to profit by the suggestions of those who adopted a different view of the subject. The result of the delay, however, has been a more settled conviction in my own mind, of the truth of the opinions I had before formed.

The object of the following pages is to endeavour to show that a febrile state of system, or what is technically denominated symptomatic fever, is always a secondary affection, the result of inflammation, and of no other cause. In this respect, the subject is of importance for the purpose of diagnosis, as it may lead us to discover the existence of inflammation in cases where it might not otherwise be suspected; that is, in cases where the local signs are not obvious, or with difficulty to be observed. It is not unimportant, likewise, in regard to practice; for although pyrexia be but a secondary state, it is capable of influencing materially both the primary disease (the inflammation which produced it) and the general system. Its influence on the primary disease is sometimes favourable, sometimes the reverse, according to circumstances that will be pointed out. It is often the cause of greater suffering to the patient than the local disease that gave rise to it; and is sometimes the more immediate cause of the fatal termination, as in the case of pulmonary consumption. Persons dying of this most fatal of diseases, are more frequently destroyed by the hectic fever (one of the forms of pyrexia), and the disorder this occasions in functions that are essential to the continuance of life, than by the absolute destruction of the lungs themselves; which are seldom so much or so extensively injured in their structure, as to be altogether incapable of carrying on their peculiar function in a manner adequate to the support of life. Hence, our efforts are most usefully directed towards controlling the hectic symptoms, for over the local disease we have little power.

Pyrexia, or symptomatic fever, as a distinct subject of investigation, as far as I am acquainted, has hitherto attracted but little notice. The Société de Médecine of Paris, a few years ago, instituted a prize question in the following terms:—"To determine the existence of idiopathic fever;" implying that, possibly, all fevers might be *symptomatic* and not *primary* affections, as commonly supposed. The affirmative of this was adopted, and ably supported, by one of their most distinguished writers, M. Broussais. It is hardly necessary for me to observe, that this is one of the points which I endeavoured to establish many years before, and which made the subject of my Inaugural Thesis at the University of Glasgow, in the year 1804, and, subsequently, of the "Enquiry into the Seat and Nature of Fever," first published in the year 1807. M. Broussais, however, differs from me in a most material point, namely, in referring the primary seat of fever to the abdomen, instead of the brain, as I had done.

Pyrexia, or symptomatic fever, then, is not, strictly speaking, one of the varieties of idiopathic fever, but an attendant on all febrile disorders, and a mere consequence of inflammation; which it serves, therefore, to denote the presence of in the system.

SECTION I.

DESCRIPTION OF PYREXIA, OR SYMPTOMATIC FEVER.

By the term symptomatic fever, is understood that general disorder of system that is observed to attend a great number of inflammations of different kinds, and wherever seated; and which is more particularly characterised by heat of skin, frequency of pulse, thirst, and foulness of tongue. By Dr. Cullen, and other modern nosologists, this state has been technically termed *pyrexia;* while, among ourselves, it is familiarly called fever (in the loose and popular sense in which this word is commonly employed), in allusion to the increased heat of the body, which is its most obvious and striking character. By that distinguished teacher and writer, the late Dr. George Fordyce[1] (the cotemporary of Cullen), it was denominated general inflammation, that is, when it arose in habits of strength; but where it took place in weak and irritable subjects, he called it symptoms of irritation. And, lastly, in the figurative language of Mr. John Hunter, symptomatic fever is said to be the effect of "the constitution sympathising with the part."

This state has never been sufficiently distinguished, by medical writers, from what has been called proper or idiopathic fever; as may be seen by attending to the definitions that have been given of the latter, and which will be generally found to include the characters of symptomatic fever, and sometimes to consist exclusively of these. Thus, Galen calls fever—"Calor præter naturam, é corde in omne corpus effusus." Boerhaave gives us the following, as the character of fever in general:—"Horripitatio, pulsus velox, calor;" and Linnæus says,—"Febris dignoscitur pulsu citato." Now, admitting that these characters are to be found in almost every case of proper or idiopathic fever, it is certain that they equally belong to symptomatic fever, or that disorder of system which attends inflammation, wherever seated, when it exists with a certain degree of violence, and to a certain extent. They afford no criterion, therefore, by which the one of these affections can be distinguished from the other.

Dr. Cullen, in his Methodical Nosology, thus defines pyrexia:—"Post horrorem pulsus frequens, calor major, plures functiones læsæ, viribus præsertim artuum imminutis." In this definition, the characters of proper or idiopathic fever are, as it appears to me, confounded with those of symptomatic fever; for the "viribus artuum imminutis," or prostration of muscular strength, is peculiarly characteristic of proper or idiopathic fever, and is, indeed, one of the signs upon which we can best rely for distinguishing it from the common febrile state that belongs to inflammation in general. This character of proper or idiopathic fever was particularly noticed by Sauvages; who observes, in his definition of this

[1] Elements of Medicine, by George Fordyce, M. D.

disease, that the prostration of strength is always greater than might be expected from the state of the vital power altogether.[1] And the same remark is made by Dr. George Pearson, in the printed syllabus of his lectures on the "Practice of Physic." It is hardly necessary to observe here, how strongly this supports the idea of the brain (which is the organ of the voluntary power) being the chief seat of disease in fever, strictly so called.

I shall proceed now to describe minutely the state termed pyrexia, or symptomatic fever, as distinguished from what is called proper or idiopathic fever.

Pyrexia, or symptomatic fever, may be divided into three stages. In the first of these, the temperature of the body is diminished; whence this has been called the cold stage, or cold fit of fever. In the second stage, on the contrary, the heat of the body is preternaturally increased, often several degrees beyond the natural standard; and this, accordingly, has been denominated the hot stage, or hot fit, and is the circumstance that has given name to the disease. In the third stage, the heat subsides again to the natural pitch, and is generally accompanied with more or less of sweating: this third stage has often been called the crisis, or critical stage, of the disease. The combination of these three stages has been termed a paroxysm of fever, especially when it terminates within the space of twenty-four hours.

The three stages now mentioned, constituting altogether, as observed, a febrile paroxysm, have been generally considered as applying to proper or idiopathic fever alone, and have entered largely into the speculations of Dr. Cullen and others, in regard to the nature or proximate cause, as it is termed, of this disease. They appear, however, to belong to pyrexia, or the febrile state in general, and by no means to apply exclusively to the train of symptoms that attend proper or idiopathic fever; so that by attending merely to the febrile symptoms in any case, without, at the same time, enquiring into local signs, it is hardly possible to determine the actual disease under which the patient is labouring. Heat of skin, frequency of pulse, and a furred tongue, are always, perhaps, to be considered as signs of existing inflammation; but the particular seat of this is commonly to be learned from the local signs, which consist in the disordered state of feeling and functions of the organ immediately affected. It is, for example, by observing pain in the head, disordered sleep, prostration of muscular strength, and other disturbance in the state of the sensorial functions, that we know proper or idiopathic fever to be present, and not by the heat of skin, furred tongue, or frequency of pulse; for these are common to all inflammations.

I shall proceed now to trace the different symptoms, in the order in which they usually present themselves.

Pyrexia, or the febrile state, is commonly, or rather, I believe,

[1] Semper virium prostratione majori, quam a virium vitalium gradu foret expectandum."

invariably, ushered in by more or less of coldness and shivering. In some instances, these are so slight as scarcely to attract notice, and consist merely in occasional momentary sensations of chilliness, felt, in particular, along the course of the spine; or, at most, are accompanied with slight and transient shiverings. On other occasions, the coldness is severely felt over all the surface: the skin is pale, and corrugated, like that of a plucked goose (*cutis anserina*); the features appear shrunk and sharp; the lips and nails are either colourless or livid; while the voluntary muscles throughout the body are in a state of involuntary tremor. There is, at the same time, a feeling of anxiety and oppression, referred to the region of the heart, attended with frequent sighing; and a considerable degree of torpor or insensibility involves both mind and body.

The pulse at the wrist, in the first or cold stage of pyrexia, is always small and feeble; generally frequent; and not seldom irregular also.

These symptoms continue but for a short period, perhaps an hour or two, and then give place to those of the second stage, or hot fit. The increase of heat is first perceived in the centre of the body, and proceeds thence to the surface and extremities. The features become again plump, and sometimes even appear swollen in some degree; while the skin altogether resumes its natural colour, or even acquires a florid hue. A more vigorous action of the heart now takes place, and the pulse at the wrist acquires both strength and fulness, often much beyond the degree of health; while it commonly retains its frequency, though in very different degrees. There is also a general sense of tension and uneasiness over the whole body.

The duration of this second stage, or hot fit of pyrexia, is very various. It may continue for a few hours only, or for several days, or weeks, or even for a much longer period, according to circumstances, which will be pointed out hereafter. When such is the case, however, the febrile symptoms never proceed with uniform violence, but, on the contrary, with exacerbations and remissions, as will be hereafter explained. When pyrexia continues for the length of time stated, it is the second stage, or hot fit, that is thus protracted.

The third stage, or crisis, is that where the symptoms are on the decline, and about to go off. This change sometimes take place rapidly, and is then generally accompanied by more or less of sweating, or other evacuation. In this case, the evacuation is said to be critical, as if it had proved curative to the disease, by carrying something noxious out of the body. But although the sweating, when it does occur, often indicates the decline, not only of the febrile state, but sometimes also of the local disease which occasioned it, this is by no means invariably the case; for the sweats often continue to be profuse for days together, without any material alleviation of either; as is seen in many idiopathic fevers, as well as other inflammations.

The three stages of pyrexia above described, commonly bear some proportion to one another. When, for example, the first stage, or cold fit, is strongly marked, the succeeding hot fit is usually violent, and generally terminates in profuse sweating. The duration of the paroxysm altogether is then commonly short, and rarely exceeds a few hours. This is observable both in ordinary inflammation, and in that variety of idiopathic fever which Dr. Cullen calls synocha, and which, from the shortness of its duration in many instances, is sometimes termed ephemera, or the fever of a day.

But if the cold stage be slight, and its approach so gradual as to be nearly imperceptible, then the hot fit which follows is generally observed to be proportionably slight; the febrile symptoms altogether are less severe, but their duration is apt to be protracted, perhaps for many days, or even weeks. Of this, we have instances in the milder kinds of idiopathic fever (typhus mitior of Dr. Cullen), as well as in other inflammations. The subsidence of the disease, in these cases, is also more slow, and accompanied usually with less sweating. These differences, in the relative state of the three stages, are common to fever (strictly so called), and pyrexia, or symptomatic fever, the result of ordinary inflammation.

Although the symptoms above described are the most obvious and striking of those that take place in pyrexia, or symptomatic fever, and are generally sufficient of themselves to characterise the affection, they are nevertheless accompanied by various others, in greater or less degree, and which it is necessary now to notice.

The change in the appearance of the tongue is one of the most remarkable of these, and the most deserving of attention; because it serves, better than any other single sign, to mark the slightest degrees of febrile action. For, in numerous instances, where neither the pulse is quickened, nor the heat of skin sensibly increased, the existence of pyrexia, or a febrile state, is shown by some or other of the changes in the appearance of the tongue now to be mentioned.

Whenever a febrile state of system arises, whatever be the cause, the appearance of the tongue undergoes a remarkable change. In most cases, its upper surface (and this only), instead of the usual florid colour which belongs to it in health, becomes covered, to a greater or less extent, with an apparent crust, or fur, as it is called,[1] that differs in thickness, colour, and others respects, at different times, so as to afford the most important indications, not merely of the existence of pyrexial, (and therefore of its cause, inflammation,) but likewise, in many cases, of the degree and quality of the inflammation, the danger that attends it, and, sometimes, even of the particular seat it occupies in the body.

In slight febrile disorders, that are apparently without danger, the fur on the tongue is of small extent, and confined to the middle

[1] It is not, however, a deposition of foul matter on the tongue, but an alteration of its natural covering, the cuticle.

and back parts of its surface; and it is also generally thin, and light in colour. If the disease be on the increase, the fur on the tongue becomes thicker and more extensive, so as often to cover the whole of its upper surface. It also acquires in such cases a darker hue; and if the disease is likely to prove fatal, the colour changes to a dark yellow, or brown, and, in certain cases, to absolute blackness. Generally speaking, the drier the tongue is, the more active and violent is the existing inflammation.

On some occasions, as in certain varieties of idiopathic fever, there is no observable fur on the tongue; its appearance, nevertheless, is very different from that of health, so as to mark sufficiently the febrile state. It becomes in such cases preternaturally red, as if deprived of its cuticle, and resembles raw flesh; and it is attended with soreness, so as to become painful, when irritating matter of any kind is applied to it.

When the liver is affected by inflammation, and the disease is so situated as to interrupt, or even to impede considerably, the passage of the bile into the intestine, the crust on the tongue is of a yellowish or bilious appearance, thus indicating the seat of the inflammation. When, again, the cerebral substance is generally or extensively inflamed, as in simple fever of the typhoid kind, the tongue turns more or less brown, according to the degree and danger of the disease. Nothing, in fact, is more characteristic of proper or idiopathic fever, than this brown appearance of the tongue; and when, as frequently happens, proper fever (brain affection) arises in the course of pulmonic inflammation, or any other, the complication is marked by the change in the appearance of the tongue here mentioned; accompanied by a manifestly disordered state of the sensorial functions, in addition to the proper pulmonary symptoms.

As a furred tongue is an unequivocal sign of existing pyrexia, or febrile action (as this again is of inflammation), so its disappearance is a criterion of returning health that is rarely found to disappoint us. And when important organs are suffering from inflammation, a patient can hardly be considered safe, so long as much foulness of the tongue remains; nor ought we, in such circumstances, to relax in our endeavours to give relief.

The crust on the tongue sometimes disappears gradually, and almost imperceptibly; at other times, the separation is effected abruptly; the crust cracks and peels off in flakes; presenting a ragged appearance, and often leaving the surface beneath excoriated and sore.

I may remark here, that the indications afforded by this state of the tongue have often been mistaken, and the appearance referred to a wrong source. It has been, and, indeed, still is, a prevailing opinion (though only of late years, since the stomach has been looked to as the great focus of disease) that the tongue is, as it were, an index to the stomach; and that its appearance is but a counterpart, or type, of that which exists in the latter organ. It seems to be imagined, that where the tongue is foul and coated, a

similar condition exists in the stomach, if not throughout the alimentary canal. It has been even thought that, in the case of aphthæ or thrush, the same white vesicular appearance that is observed in the mouth and fauces, extends, at times, throughout the whole alimentary canal; and this is considered to be proved by a similar appearance being observed at the extremity of the rectum. Of this, however, there is no proof. The peculiar appearance observed in the mouth and fauces, in cases of aphthæ, is an affection of the skin, and of the cuticle covering it; but these structures can hardly be traced beyond the fauces. Inflammation no doubt often exists, in such cases, throughout the mucous membrane of the intestines; as indicated by pain, and soreness, and an abundant and acrid secretion from the inflamed surfaces: but this is very different from the fur on the tongue that accompanies the febrile state simply, and which commonly takes place without any corresponding disorder of the stomach or intestinal canal. The tongue, in short, is an index to the sanguiferous system, the excited and disordered state of which it clearly serves to mark; and not to the stomach, as commonly supposed. This is a point of some importance, practically considered. The employment of purgatives, and other means directed particularly to the stomach, on the latter supposition, is, doubtless, often highly useful, as tending to relieve inflammation when it really exists, and, by so doing, clears the tongue; but the objection is, that such treatment is not always adequate to the purpose, and requires the aid of more active remedies, and of blood-letting more especially, which cannot always be safely dispensed with.

In addition to the symptoms already mentioned, as accompanying pyrexia, or symptomatic fever, the different secretions are observed to be disordered, and that variously; sometimes being in excess, but more frequently diminished, or altogether suppressed.

The secretion of saliva, as well as that from the mucous membranes of the mouth and fauces, is generally lessened; hence the dryness of those parts, with a desire for liquids. The secretions from the rest of the alimentary canal, as well as those of the organs in immediate connection with it, and which pour their fluids into it, as the pancreas and liver, are commonly diminished; and thus costiveness is induced. The reverse of this sometimes takes place, and purging is the consequence. Now and then there is but little change observed in the state of these secretions.

The kidneys appear, in all cases of pyrexia, to have their function disturbed; and a corresponding change is observed in the state of the urine. At first, it is scanty and high coloured, and loaded with saline matter, so as to irritate painfully the passages through which it is discharged. As soon, however, as the febrile action begins to decline, the urine is secreted in larger quantity, and is less highly coloured; and, though generally transparent when first passed, it becomes turbid as it cools, and gradually deposits a sediment; which is sometimes of a brick-dust colour (*lateritious*), sometimes of a bran-like (*furfuraceous*) appearance. This deposit

takes place more or less perfectly at different times, so as to mark the more or less perfect subsidence of the febrile action.

The urinary secretion, indeed, is of nearly equal importance with the tongue in indicating the state of the general system in cases of inflammation, and merits our particular notice. This was well understood by the older physicians, though of late it has been neglected, in favour of the hepatic function; which affords signs, however, that are far more uncertain and equivocal.

In the skin, an unusual degree of dryness commonly takes place at first; and which is partly to be ascribed to diminished secretion, partly to the quicker evaporation of the cutaneous exhalation during the hot fit. After a time, this dryness of the skin is followed, in most cases, by sweating, which is more or less general, as the circulation happens to be either generally or partially increased. It is probable that changes no less striking would be observed in the state of the other secretions, had we the same ready means of detecting them.

The absorbing vessels, as well as the blood-vessels and exhalants, appear to be in a state of increased and disordered action in pyrexia; hence the body wastes as the disease proceeds; so that in long-protracted febrile disorders of every kind, the greatest emaciation takes place. Other functions are observed to be more or less disordered, during the continuance of pyrexia, or the febrile state; that of the stomach, in particular, is apt to be impaired, and often wholly suspended; there is neither the usual appetite for food, nor is digestion properly performed; and thus an additional cause of weakness and emaciation is afforded by the interruption given to the supply of the system.

The general feelings of the patient are uncomfortable, if not actually painful. The sleep is diminished, unsound, and unrefreshing. The muscular strength is always more or less impaired; the natural result of the unequal distribution of the vital power, and its too great expenditure in the excessive action that is going on in the whole vascular system. The inability for muscular exertion, however, that takes place in ordinary cases of pyrexia, or symptomatic fever, is very different from that remarkable prostration of strength observed in idiopathic fever, and which appears to proceed from the oppressed and disordered condition of the brain, essential to this disease.

The degree of pyrexia, or febrile action, present in any case, is not always a sure criterion by which we can judge of the actual degree and danger of the primary disease. In irritable states of the system, as in children particularly, slight inflammations, such as are attended with no danger, and but little inconvenience, will nevertheless often give rise to violent febrile symptoms. On the contrary, in torpid and inirritable habits, such as are met with in advanced life, the reverse is commonly observed. The primary local disease may, in such cases, be making a rapid progress, so as to endanger life, with but little general arterial excitement.

In certain diseases of great danger, the pulse is often found, for a

time, to vary but little from the natural state; nor is the temperature of the body materially increased. This is observed occasionally in cases of severe pneumonia; and likewise in many fevers of the kind usually termed malignant. The tongue, however, in these cases, by its thickly crusted and parched state, and by its dark brown colour, generally indicates, with sufficient clearness, the impending danger.

From what has been now said, it is evident that, although much useful information is to be derived from attention to the general febrile symptoms—that is, from the state of the pulse, heat of skin, and others—they are not in all cases wholly to be relied upon. Thus, we cannot always form a just estimate of the degree, and probable termination, of idiopathic fever, merely from examining the pulse, or heat of skin, or even the appearance of the tongue, in the early part of the disease; but must look particularly to the way in which the functions of the organ primarily affected (the brain) are performed, as shown by the state of mind, of sensation, and of the voluntary power. And so in regard to other inflammations.

On the other hand, the febrile symptoms are often violent where the primary inflammation is slight, and the danger comparatively small. Of this we have frequent instances in pleurisy, acute rheumatism, and, likewise, in that variety of idiopathic fever, termed the inflammatory (synocha); in all of which the heat of skin is great, the pulse full and strong, and the tongue thickly coated; and yet, under proper management, there may be little real danger.

Pyrexia, being always a secondary affection, is in strict dependence upon the primary disease that originally produced it. If this cease, either spontaneously (as it often does), or be removed by art, the febrile action ceases also, or immediately begins to decline. This is the case, whether the primary disease be acute or chronic in its form. Thus, in the hectic fever occasioned by cancer, or by an incurable disease of a large joint, the hectic disappears as soon as the diseased part has been removed by amputation—unless, as is often the case, the febrile symptoms should be kept up by disease in the lungs, or some other organ.

The converse of what has just now been stated, is not, however, necessarily true. The febrile symptoms induced by inflammation may either cease spontaneously, or be removed by art, while the original disease (the inflammation) remains; and that in a degree that may ultimately prove fatal by destroying the texture of the part. Of this, numerous instances occur in the case of pneumonia. The heat of skin, the frequency of pulse, and even the fur on the tongue, may wholly, or in a great measure, disappear; yet some degree of inflammation may remain behind in the lungs, sufficient to lay the foundation of pulmonary consumption at a future though remote period. As a general rule, therefore, we ought not to limit our efforts to the removal of the pyrexia, or febrile state, but to watch narrowly the local symptoms, not resting satisfied till these have disappeared, as well as the general disorder of system. From

want of attention to this point, the most fatal chronic maladies often follow as the consequence of imperfectly cured inflammation.

In the greater number of cases, however, the cessation of the febrile symptoms is a proof of the subsidence of the primary affection, the inflammation that caused them.

SECTION II.

OF THE STATE OF THE BLOOD IN PYREXIA.

If, when the body is in a febrile state, blood be drawn in a full stream from either an artery or vein, and received into a somewhat deep and narrow vessel, its properties are generally found to be materially changed. It soon exhibits a bluish tint on its surface, and that while it is still fluid. It is longer, too, in undergoing coagulation than in health; and, on becoming solid, the surface, instead of the usual florid hue of health, is covered with a coat or crust, of more or less firmness and transparency, and which is usually termed the inflammatory crust. This appears to be owing principally to the slower coagulation above mentioned, in consequence of which the red particles have time to subside to the bottom; but, in some degree, probably, to a change in the nature of the blood altogether.

Other differences, in regard to the figure of the consolidated mass, and its firmness or tenacity, are observed, according to circumstances to be presently mentioned. Sometimes, for instance, the surface of the coagulum is flat and extensive, so as to occupy the whole area of the vessel in which it is lodged. In this case, the crust is semi-transparent, and of a gelatinous texture, like half-melted glue. The blood is then said to be sizy. This is usually found to be the case in simple idiopathic fever, where the prostration of muscular strength is considerable, without any very acute pain in the head, and where there is no sign of inflammation in any other organ than the brain. At other times, the crassamentum is firm in texture, and contracted into more or less of a globular shape, with a hollow or cup-like appearance of the surface, and often with curled or crimped edges. The inflammatory crust is then observed to be opaque, tough, and yellowish, not unlike buff-leather. This is found to be the case in long-continued inflammations, and especially of membranous structures. It is particularly observable, also, in pulmonary consumption, and affords a very ill omen in regard to the result.

The inflammatory crust on the blood seldom makes its appearance till the inflammation has existed for at least a day or two; and it is commonly the more considerable the longer the disease has subsisted. Accordingly, it is often wanting on the first or even the second bleeding; but is almost sure to appear afterwards, if the disease proceeds. This would seem to show, that a so-called inflammatory appearance of the blood is not the immediate effect of

the inflammation itself, but of the pyrexia, or febrile state of the system which this produces.

The inflammatory crust on the blood may be prevented from taking place by trivial circumstances, although the disposition to it really exists. Thus, if the blood flows in a very small stream, or trickles down the arm, or is received into a broad and capacious vessel, so as to expose a large surface to the air, it coagulates so quickly that time is not given for the red particles to subside. On this account, blood drawn by scarification and cupping glasses rarely shows the inflammatory crust.

The general inference to be drawn from what has been now stated, in regard to the inflammatory crust on the blood, is, that although its appearance is to be considered as evidence of existing pyrexia, or symptomatic fever, (and, therefore, of inflammation, as the cause of this, as I shall endeavour to show hereafter,) yet the absence of such an appearance of the blood is not a decisive proof of the absence of inflammation—a point that can only be determined by other signs.

SECTION III.

OF THE VARIETIES OF PYREXIA, OR SYMPTOMATIC FEVER.

Many circumstances tend to vary the character of pyrexia, or symptomatic fever, and which it is necessary to notice, as a knowledge of them will be found to illustrate the diagnosis, as well as to influence the treatment of the primary disease.

When pyrexia is protracted beyond the space of a few hours, it is the second or hot stage that constitutes the principal part of the affection; and it is in this, therefore, that the varieties are chiefly seen.

1st. Pyrexia may exist in all degrees. In the slightest degree, the pulse is but little, if at all, altered from the natural state in any respect; the temperature of the skin is hardly sensibly raised; and the tongue is but thinly covered, on its upper surface, with a whitish fur or crust, which is confined to the middle and back parts of it. In the more violent degrees of pyrexia, the symptoms altogether are proportionably severe. The pulse becomes much increased in frequency; the heat of the body is greatly augmented; and the tongue is thickly and extensively encrusted. The different functions are likewise all disturbed in a higher degree.

2d. The habit of body, as regards both strength and irritability, has a considerable influence in modifying the character of pyrexia. When this takes place with any degree of violence in vigorous subjects, the pulse becomes full and strong, as well as frequent. The increased frequency, however, is less than in other circumstances, seldom going to more than twenty or thirty beyond the natural range. The heat is universally increased; and there is a feeling of tension and soreness throughout the body. The urine is scanty,

hot, high-coloured, and offensive to the smell. The crust on the tongue is thick, white, and dry, and covers its whole upper surface. The blood, when drawn, exhibits a large proportion of crassamentum, which is firm in its consistence.

In weak and irritable persons, on the other hand (the primary disease being the same), a considerable difference is observed in the character of the febrile symptoms. The pulse acquires greater frequency, rising often to 130 or 140 in a minute. Instead of being full and strong, as in the former case, it is small and weak, being easily stopped by the pressure of the finger. The increase of heat is partial, and confined principally to the central and deeper-seated parts; not extending to the extremities, unless the temperature of these be supported by artificial means. The tongue, though as much furred as in the other case, is more frequently moist than dry. Blood drawn, in these circumstances, has a large proportion of serum; and the coagulum is less firm in its texture.

3d. Age has considerable influence in modifying the character of, or rather the tendency to, pyrexia. Very slight inflammation, such as would have little or no effect on persons advanced in life, is sufficient to excite a high degree of febrile action in infants. Females resemble infants in this respect. The degree of pyrexia present, therefore, as before observed, is not always a test of the actual violence or danger of the inflammation which produces it.

4th. The character of pyrexia is varied also by duration. For the first few days it usually proceeds with much uniformity, or in a continuous form. After this, a considerable abatement of symptoms usually takes place, once, at least, in twenty-four hours, attended with sweating; which is soon again followed by an exacerbation. This is best seen in acute rheumatism; but it appears also in other inflammations, when protracted beyond a week or two.

Pyrexia sometimes continues for several months, being kept up by the continuance of the primary disease; it is then found to undergo a more considerable change of form, assuming a more decidedly remittent character. The pulse becomes small, quick as well as frequent, and hard or cork-like to the touch. The heat of skin is partial and unequal, being the greatest, and most sensibly felt by the patient, in the palms of the hands and soles of the feet; the cheeks, too, are frequently flushed in circumscribed patches. Slight chilly fits occur about noon, and are followed, towards evening, by a hot and parched state of the skin, which continues through a great part of the night, and is attended with much restlessness. Towards morning, profuse sweats break out, generally during sleep, and continue for some hours. The heat and frequency of pulse then decline, and continue moderate during the early part of the day; after which, the same train of symptoms is renewed; and so on, from day to day, though with unequal severity. I hardly need add, that the variety of pyrexia just described, constitutes what is termed hectic fever, as observed in cases of pulmonary consumption, diseased joints, and many other

long-protracted and incurable diseases. It is merely pyrexia in a chronic form.

5th. One variety of pyrexia is of so peculiar a nature, as to demand particular notice; and the more so, as it admits of a peculiar use of remedies; I mean the regularly periodical form, as observed in fevers of the intermitting kind; for in this, as in all other cases, the pyrexia, or febrile state, appears to be symptomatic of local disease. In this case, the paroxysms of febrile action are pretty regular in their times of recurrence, as well as in their duration, which is always considerably within the limit of twenty-four hours. The intervals between the paroxysms are also distinctly marked, as being in a great measure free from pyrexia; yet not absolutely so; for the tongue is always observed to be more or less coated, even in the most perfect of the intermissions; while there are often other signs of febrile action being present in a low degree. In this form of pyrexia, the three stages—the cold, the hot, and the sweating stage, or crisis—are all, in general, distinctly marked.[1]

Several varieties have been noticed in the character of periodical pyrexia, or, as it is commonly termed, intermitting fever. Thus, in some cases, the accession of febrile symptoms occurs daily; in others, every other day; and in others, again, with an interval of two days; and thus are formed the quotidian, tertian, and quartan, types of intermitting fever. Still longer intervals have been noticed; but it is doubtful whether such occur with any regularity. Other diversities in the character of intermittents have been observed; but a fitter occasion will offer for their consideration hereafter, when treating of the varieties of idiopathic fever.

6th. The nature of the part, or organ, primarily affected, has a great effect in modifying the character of pyrexia; the want of attention to this has led occasionally to a misconception of the nature of some diseases, and sometimes to an improper mode of cure.

When inflammation, for example, arises in a part that possesses but a moderate share of sensibility or irritability, and which is not very strongly or extensively associated by sympathy with the rest of the system, the pyrexia to which it gives rise is simple in its form, and seems to consist in little more than a state of increased action of the heart, and general vascular system. The pulse is full and strong, and but moderately quickened; the heat of the body is pretty universally increased; while the other functions are comparatively but little disturbed. Such is the case with the ligamentous structure in ordinary cases of acute rheumatism; with the pleura, when inflamed; and in inflammation of the parenchyma of the liver, and of the cellular texture in general.

But if the part inflamed be an irritable one, or possess a high degree of sensibility, such as the stomach, small intestines, uterus,

[1] The intermissions of febrile action are sometimes nearly as regular in some other diseases as in intermittents, strictly so called. This is occasionally observed in phthisis pulmonalis, which is thus liable to be mistaken for intermitting fever.

or skin, then the febrile symptoms are of a different character. The pulse becomes small, hard, frequent, and quick; there is a feeling of great anxiety; delirium also is apt to arise; great disorder is observed in the state of all the functions; with general prostration of strength. These peculiarities attend such inflammations, even in the strongest subjects; and the fact is of importance to be noted, since the apparent debility has often led to the opinion that blood-letting is not admissible, where, in fact, it is imperiously called for. The distinction between such temporary depression of strength (the immediate result of the disease), and real constitutional debility, is to be drawn chiefly from the general circumstances of the patient immediately previous to the attack, as well as from the duration of the disease altogether.

The pyrexia occasioned by inflammation of the heart, has likewise its peculiarities, a knowledge of which assists us in the diagnosis of the disease. In this case, the irritability of the organ is greatly increased, and its contractions, in consequence, much quickened. The pulse, accordingly, is extremely frequent, and, at the same time, necessarily small and thready. Sometimes it is irregular also; and, occasionally, there is syncope; but neither of these takes place so frequently as the definitions usually given of carditis would lead one to imagine. I have seen many well-marked cases of the disease, where the pulse, though extremely quickened, was not at all irregular, and where no tendency to syncope was observed.

There is one condition of system, occasionally induced by inflammation of the heart, which has been little, if at all, noticed by writers; this is, an entire failure of the pulse at the wrist, yet without that suspension of the functions of the brain that attends ordinary syncope; from which it is to be distinguished by the circumstance just mentioned, of the patient retaining a perfect consciousness, and by the severe pain felt in the region of the heart. The tongue is furred, as in other inflammations; the body is pale and cold; the countenance expresses great anxiety and distress. The failure of the pulse here appears to be owing to the strong disposition to contract in the muscular structure of the heart, occasioned by the inflammation, and which seems to induce in it a very near approach to actual spasm. The general circulation still goes on, but in a very imperfect manner; sufficiently, however, to supply the brain with blood, and to enable it to continue its functions.

The treatment of the disease, under such circumstances, is somewhat embarrassing, from the difficulty there is in abstracting blood where the general circulation is almost at a stand. A remarkable instance of this kind fell under my observation, several years ago, in consultation with my friend, Mr. Pettigrew. The patient was a strong man, between forty and fifty years of age; and accustomed, as a porter in the East India warehouses, to lift heavy weights. In the course of his labour, one day, he complained of pain in the left side of the thorax, and which became gradually so severe as to

oblige him to leave his work. When I first saw him, in the evening, the pain in the region of the heart was so acute as to make him cry out; the face was pale, his look anxious, and the hands cold. I was surprised, on examination, to find there was no pulse to be felt at the wrist; nor could I distinctly feel the heart beat, on applying my hand over it. The tongue was considerably furred.

Upon reflection on the nature of the symptoms, it seemed unlikely that, in such a subject, the disease should be simply spasmodic; while the state of the tongue appeared to indicate inflammation. I resolved, therefore, to treat it as such. A vein was opened in the arm; but, as might have been expected, little or no blood flowed. Cupping, therefore, was resorted to, and, with difficulty, a few ounces of blood were thus obtained. By this, however, the pain was in some degree relieved, and the pulse became perceptible at the wrist. After an hour or two, a vein was again opened in the arm, and the blood now flowed, though slowly; and when about four ounces were drawn, syncope took place. The bleeding was repeated several times, at intervals of a few hours, till between thirty and forty ounces of blood, altogether, were drawn. It was observed, that after each bleeding the volume and strength of the pulse were sensibly increased; so that at the end of thirty-six hours, it had recovered its natural state, and the disease was removed. Blistering the chest was also employed. It is probable that opium would be useful in such a case, both by relieving pain, and lessening the disposition to contract in the heart; and hence it would be disposed to admit a larger quantity of blood into its cavities.

When the lungs are extensively and violently inflamed throughout their substance, the character of the attending pyrexia is altered in a different way. The swelling of the inflamed parts, by pressing upon the air-cells, and the minuter ramifications of the pulmonary artery and veins, at once prevents the free admission of air into the lungs, and impedes the circulation of the blood through them. The blood, therefore, is not sufficiently influenced by the atmosphere, but retains its venous character; hence the livid hue of the skin in various parts; it fails also to acquire the stimulant properties of arterial blood in the due degree. The heart and vessels, in consequence, are not sufficiently excited; whence the pulse is soft, and easily compressed. The general circulation is feebly carried on; and the animal heat is diminished. The tongue, also, in this case, instead of being white on its surface, becomes covered with a fur of a brown colour, of different shades, and of different thickness, according to the violence and danger of the disease. The blood, when drawn, is dark coloured, and coagulates loosely. The brain, likewise, suffers from the same cause, stupor taking place in a greater or less degree.

We have here, again, a practical proof of the necessity of the distinctions pointed out being attended to. If we were to attend solely to the pulse, or to the general depression of the system, in violent inflammation of the lungs, we should be deterred from the

abstraction of blood; yet in hardly any case is this evacuation more necessary, provided it be resorted to at a sufficiently early period—a point of paramount importance.

But there is no organ, perhaps, which, when inflamed, so greatly modifies the character of the attending pyrexia, as the brain; the reason of which is sufficiently obvious. The great influence exerted by this organ over the whole body, enables it, when extensively diseased, to produce universal disorder throughout the system; so as to make it often difficult, amidst the multiplicity of symptoms, to discover the primary seat of disease.

When, for example, inflammation arises in the cerebral substance, (as I have assumed to be the case in all idiopathic fevers,) not only are the proper or immediate functions of the brain, namely, sensation, voluntary motion, and intellect, more or less disturbed, giving rise to the peculiar or pathognomonic symptoms of idiopathic fever, but the general vascular action is likewise deranged, and that variously, according to the particular seat, extent, and violence of the disease in the brain, so as to vary greatly the form and character of the attending pyrexia. Thus, in some fevers, the pulse is much quickened; in others, the reverse; while there are some in which it is but little altered from the natural state, even where the disease is of the most formidable nature; so that the pulse is often a very insufficient index of the violence or danger of the disease. There is a quality of the pulse, however, belonging to idiopathic fever in general, when of a simple nature, which distinguishes this from other diseases; this is, a thrilling, tremulous, or vibratory state, more easy to be felt than described, and which is attended with peculiar softness. The tongue, instead of perfect whiteness, is of a brownish hue, and the more so in proportion as the disease is of a more formidable description. The crassamentum of the blood is flat and extensive on its surface, and covered by a semi-transparent or gelatinous crust (*size*, as it is termed), while its texture is rather loose than firm. These states of the pulse, the tongue, and the blood, are highly characteristic of fever in its simplest form—that is, when it exists uncombined with other inflammations.

The state of the skin, and of the secretions in general, is peculiar in idiopathic fever, as well as the other circumstances described above. The heat of the surface is great, and of a peculiar pungent kind; and very different from what is observed in other diseases. The secretions show an unusual tendency to decomposition or putrefaction, and which tendency increases as the disease proceeds. This is indicated by the gradual accumulation of black and offensive sordes about the mouth and teeth, rendering the breath offensive; and by the fetor of the various discharges.

The form of pyrexia now described, as attending proper or idiopathic fever, is more frequently unaccompanied by pain in the organ primarily affected, than that which accompanies other inflammations; a circumstance which, as already observed, is to be ascribed in part to the natural insensibility of the brain, and

also to the confusion of intellect which disease in this organ is apt to occasion. In many cases of idiopathic fever, the pain in the head is considerable at first, but subsides in the course of a few days, and is not afterwards complained of. Hence, the general disorder of system only being seen, the local nature of the disease is readily overlooked.

7th. When inflammation, from its extent or violence, or from its being seated in a part of great importance to life, begins to threaten a fatal termination, the approach of this is commonly discoverable in the state of the febrile symptoms. The heat of skin continues, but is partial and irregular; the sweats are profuse and clammy; the pulse becomes more frequent, small, and weak; the fur on the tongue, thicker and darker coloured; while the general distress of the patient increases. These all take place in addition to the pain and interruption of function of the organ primarily affected.

Again, if the inflammation has terminated, or is about to terminate, in the death of the part (gangrene), the symptoms above described are all aggravated in the highest degree. The pulse is extremely hurried, small, and irregular; cold sweats appear on the forehead, and successively in other parts; the tongue turns still darker and drier, and becomes more thickly encrusted; the countenance expresses extreme anxiety, though often no actual pain is complained of; tremors of the voluntary muscles ensue (*subsultus tendinum*); the diaphragm is convulsively affected (hiccup); the mind wanders; the urine and fæces are discharged involuntarily; and death soon follows.

SECTION IV.

OF THE CAUSES OF PYREXIA.

The term symptomatic fever implies that it is to be regarded as a secondary state—a consequence, merely, of some prior affection, that disorders, by irritation, the general system, in the manner above described. But it is important to observe, that all kinds of irritation have not this effect. It seems, indeed, to be that kind of irritation which inflammation produces, that is the chief, if not the sole, cause of pyrexia, or the febrile state.

Pain simply, however acute, does not give rise to febrile action. Thus the pain attending parturition—that which is induced by the passing of gall-stones through the ducts—as well as many other pains of a purely spasmodic kind, so far from inducing a febrile state, often render the pulse slower than in health, and are attended, for a time, with a diminution of animal heat. But when, as often happens in the cases just alluded to, the pain, from being intermitting or remitting in its character, (as it usually is when of a simply spasmodic nature,) becomes continued, with increase of heat, and tenderness to the touch in the part affected, (signs which always

indicate the accession of inflammation,) the pulse immediately begins to quicken, and all the other signs of pyrexia soon follow.

Local irritations of various kinds, that do not proceed from, nor are accompanied by, inflammation, may give rise to great general disorder throughout the system, especially in the cerebral functions; but without, at the same time, disturbing the sanguiferous system, that is, without inducing pyrexia. Thus epilepsy, or convulsions, as they are called, in infants, are often brought on by disorder of the stomach and bowels, and by the irritation of teething. In like manner, when tetanus follows wounds or other injuries, it is generally in cases where the inflammation in the injured part is very inconsiderable; and, indeed, in many instances of this sort, the spasmodic contractions do not appear till the wound is entirely healed, and when, of course, all inflammation has subsided. In these cases, pyrexia, or a febrile state, is not observed.

On the other hand, inflammation, when either violent or extensive, has almost invariably the effect of producing pyrexia. Nor does this occur only when the great or vital organs are the seat of the inflammation. The same effect follows, where the simplest structures are inflamed to any considerable extent. Thus we find that ligamentous inflammation (gout and rheumatism)—a copious eruption of pimples on the skin, however simple in their nature, and likewise extensive inflammation of the mucous membrane lining either the respiratory or alimentary passages—are each productive of febrile action, and that in proportion to the extent and activity of the inflammation present.

It is clear, therefore, that inflammation is a general and powerful cause of pyrexia, since no violent or extensive inflammation takes place without inducing it. And one is naturally led to conjecture that it is the chief, if not the sole, cause of febrile action. In order to determine this point, however, it is necessary to enquire, whether pyrexia is ever met with, distinct from, and unaccompanied by, inflammation as its cause.

It cannot be denied, that many instances occur of a febrile state of system, in which, for a time at least, the existence of topical inflammation is by no means obvious. Such cases have been called general or universal diseases, or diseases of the whole system; as if no part or organ suffered primarily or essentially more than the rest, or was immediately concerned in the production of the febrile state. The case of proper or idiopathic fever has been especially relied upon in proof. A little consideration, however, will, I think, show, that this is far from being conclusive on the subject; for, in the first place, it may be observed, that inflammation often exists where it is not suspected. It is by no means uncommon for indubitable traces of pre-existing inflammation to be met with after death, of which no suspicion was entertained during life; and that even in the most important organs, as the brain and lungs, as well as many others. This may be accounted for on different grounds: as, first, the not distinguishing sufficiently between the primary and essential, or pathognomonic, symptoms of a disease, and such as are only

secondary, or of casual occurrence; the latter being often more striking, and more distressing to the patient, than the former; and hence both the seat and nature of the disease are liable to be overlooked. Inflammation of the liver is thus frequently not noticed, from the attention of the patient being directed exclusively to the disordered functions and feelings of the stomach, which, under the convenient name of dyspepsia, are considered and treated as the primary disease; often with the effect of aggravating the hepatic inflammation. In like manner, inflammation of the brain, in children more especially, is frequently ushered in by vomiting; and is hence considered as an affection of the stomach merely, and treated as such, perhaps by stimulants or opiates, till the occurrence of more strongly marked cerebral symptoms denotes the true nature of the disease—a discovery that is sometimes made too late.

Another cause of inflammation being frequently overlooked, is the small share of sensibility possessed by many internal organs, which, on that account, may be actively and even fatally diseased, without being attended with pain sufficient to attract notice. The liver and lungs both furnish instances of this, but still more the brain. This organ, though the source of feeling to other parts, is itself among the most insensible, and its diseases, in consequence, the most liable to be overlooked. It is not by pain in the head, therefore, that we are enabled to judge of the degree or nature of disease in the brain, but by the disordered state of the cerebral functions. In the most acute instances of phrenitis, the disease often proceeds to a fatal termination without pain in the head being at all complained of. So also, in many cases of idiopathic fever, the pain in the head is inconsiderable, and quite disproportionate to the other symptoms of the disease; or, if the pain in the head be at first severe, it ceases to be complained of in the more advanced stages of the disease. This may be partly ascribed to the disturbed state of the mental function in those cases; but is referable, also, in part, to the natural insensibility of the organ itself.

The case of idiopathic fever, therefore, that has been so much relied upon, as showing that pyrexia, or a febrile state of system, may take place without the presence of inflammation, does not appear to furnish conclusive evidence on the point; because, as I have just stated, inflammation may be going on in the brain, although little or no pain be complained of at the time. It is certain, however, from the disturbed state of the sensorial functions, (admitted to be present in every case of proper or idiopathic fever,) that active disease of some kind is going on in the brain; and that such affection consists in inflammation, is a matter of fair inference, from the general character of the symptoms, as compared with those induced by other acknowledged inflammations in this part; while this conclusion receives all the support from dissection that could reasonably be looked for, as I have elsewhere endeavoured to show.[1]

[1] Enquiry into the Seat and Nature of Fever.

It has been said, that although it be admitted that pyrexia, in a great proportion of instances, appears to be the result of inflammation, yet that cases are not unfrequently met with, where the febrile symptoms precede, rather than follow, the inflammation, and cannot then, of course, be caused by this. Thus Dr. Cullen, in describing pneumonia, observes—"That the disease almost always comes on with a cold stage, and is accompanied with the other symptoms of pyrexia." "Sometimes," he adds, "the pyrexia is, from the beginning, accompanied with the other (the local) symptoms; but sometimes it is formed for some hours before the pain is felt."[1] It may be replied to this, that pain is equivocal as a sign of inflammation, which, in this, as in the case before alluded to, may have existed, though not noticed prior to the febrile attack.

A number of other causes, not very obviously connected with inflammation, give rise to febrile action at times. The use of mercury, to a certain extent, is an instance of this; as proved by the heat of skin, quickness of pulse, and furred tongue, as well as the inflammatory crust on the blood, which a long-continued use of this medicine never fails to produce. Antimony has also the same effect, after being freely used for several days—a fact that seems to be less generally known. These substances appear to be taken up by absorption, and, being carried into the blood-vessels, probably act on these, so as to excite them into increased action, if not to produce actual inflammation on their internal surfaces. The power of both mercury and antimony, to excite inflammation on external surfaces, is well known; and one can see no reason why they should not produce the same effect on the irritable internal membrane of the heart and blood-vessels.

The states of pregnancy and menstruation are often accompanied by clearly marked febrile symptoms, and the blood drawn in such cases exhibits a buffy coat. The condition of the general system in both these states is, in many respects, analogous with that which inflammation produces; while the uterus itself exhibits signs of inflammation, or, at all events, a near approach to it, in the pain, and heat, and throbbing, and sense of distension in the uterine region, that constantly attend these states in greater or less degree. It would be going too far, perhaps, to say, that pregnancy and menstruation are, at all times, actual states of inflammation; but it is impossible not to see a striking similarity between them. For, besides the symptoms described above, the same increased action and distension of vessels take place, both in pregnancy and menstruation, that belong to inflammation in general; and, to all appearance, with the same final object in view, namely, either the growth of new solids, or the production of new fluids—processes that are always attended by the condition of vessels here mentioned, and which are equally observed in inflammation, and in the natural states alluded to. Menstruation, in fact, in many females,

[1] First Lines, § 326.

is a state nothing short of actual uterine inflammation, and calls for an appropriate treatment.

Various causes of excitement to the sanguiferous system, give rise to a state in some respects similar to pyrexia. Such, for example, are violent exercise, the hot bath, the use of alcohol, and various stimulating drugs, as ammonia, spices, and many others; all of which powerfully excite the heart and arteries, so as to produce increased circulation of the blood, and raise the temperature of the body altogether. It may be questioned, however, whether the state thus induced is really of the nature of pyrexia; for it is not attended by that general disturbance of functions, and the coated tongue, that are observed in genuine symptomatic fever. They appear rather to be cases of simple vascular excitement, which soon subsides again; with no other result than a degree of weakness or exhaustion, that naturally follows over-excitement of any kind. These same causes, however, if carried to great excess, are capable of producing actual inflammation; and real pyrexia may then follow. Thus it is that a debauch in eating and drinking is often succeeded, on the following day, by signs of inflammation in the mucous membrane of the œsophagus and stomach (cardialgia), accompanied by the ordinary febrile symptoms. In like manner, alcohol, in all its forms, as long as it is used moderately only, seems simply to excite the vascular action of the brain; thereby first giving greater activity to the sensorial functions, and subsequently, if carried further, inducing that temporary derangement of intellect which we call intoxication. But the same stimulus, if carried to great excess, is often followed by actual inflammation of the brain, which manifests itself sometimes in the form of phrenitis, sometimes of ordinary idiopathic fever, and with the usual pyrexial symptoms.

SECTION V.

OF THE NATURE OF PYREXIA, OR SYMPTOMATIC FEVER.

The characters of this affection are most strikingly displayed in the sanguiferous system, the heart and blood-vessels, the condition and actions of which are always observed to be greatly changed, though differently in the different stages. In the first, or cold stage, of pyrexia, the capillary vessels of the skin are evidently in a state of constriction, which prevents the free transmission of blood through them, and must of necessity occasion more or less impediment to the general circulation. This constriction of the capillaries was not inaptly denominated spasm, by Dr. Cullen, and was considered by him as forming an essential link in the chain of morbid phenomena of proper or idiopathic fever. It appears, rather, to be only a part of that general disorder of system which inflammation is at all times so apt to induce, and by no means to

belong peculiarly, or exclusively, to idiopathic fever. We find it, accordingly, taking place as a consequence of all violent inflammations; in combination with the local symptoms that arise out of the disordered state of feeling, and of function of the organ primarily affected.

The heart, during this cold stage of pyrexia, acts feebly, and often irregularly, as discoverable in the state of the pulse. There is a sense of oppression about the heart, as if the organ were unable to propel the load of blood that is thrown back upon it, in consequence of the contraction of the capillaries. This is accompanied by a disposition to sigh frequently, giving the appearance of mental distress; and which, probably, was the occasion of the application of the term *anxiety* to this peculiar feeling—a feeling that is not entirely removed till the circulation is fully restored in the extreme vessels; or, as Dr. Cullen expresses it, "till the spasm of the capillaries is removed." It is evident, both from the feebleness of the pulse, and the diminution of animal heat, that the force of circulation is greatly impaired during this, the cold stage of pyrexia.

In the second, or hot stage, a very different, and, in most respects, an opposite state of things prevails. The action of the heart and arteries, which was so much depressed in the cold fit, is now increased, even beyond the natural pitch; as evinced by the greater fulness, strength, and frequency of the pulse, and the augmented temperature of the body, together with all the other marks of increased circulation.

When the habit of the patient is strong, the increase of arterial action above noticed, beginning, as it does, in the central parts, is gradually and uniformly extended over the whole body; as is observed in cases of pleurisy and acute rheumatism, and in that variety of idiopathic fever that is called inflammatory (synocha, of Cullen), and in various other active inflammations. But, in weak subjects, the increase of vascular action that takes place in the hot fit of pyrexia, is partial and unequal; the vital power being apparently insufficient for supporting a general increase of action throughout the system. The more distinct vessels act feebly; and the extremities, in consequence, are pallid and cold, unless their temperature be preserved by artificial means. The pulse at the wrist, under these circumstances, is commonly frequent, but, at the same time, small and weak.

This increase of vascular action, as observed in the second stage of pyrexia, the hot fit, is of an unnatural or morbid kind, as may be inferred from the altered state of the secretions, especially observable in the urine; from the interrupted process of nutrition; and from the disturbance of various other functions, as already noticed, as well as from the changes induced on the blood itself. The same increase of action that takes place in the sanguiferous system, seems to exist also in the absorbing vessels; hence the progressive wasting of the body, which is observed constantly to follow the continuance of febrile action, from whatever cause it may proceed.

In the third, or last stage of pyrexia, (the crisis, or termination of the paroxysm,) the preternatural excitement of the sanguiferous system begins to decline, and gradually subsides into the natural state; while the absorbents, which, during the second stage, appeared to be acting in excess, cease now to do so, and even fall below their natural degree of action; and hence it is, probably, that anasarca, or general dropsy, so often follows the subsidence of the febrile state, when this has been either long continued or severe.

If the arteries be examined after death, in cases where the febrile action has been violent during life, their lining membrane, as well as that of the heart and larger veins, is found to be highly reddened, so as to resemble the tunica conjunctiva of the eye in an inflamed state, and the substance of the vessel altogether is observed to be thickened. This I have repeatedly witnessed in persons dying of pulmonary consumption, when the hectic fever has continued violent to the last. In a case of acute rheumatism that terminated fatally, in consequence of what is called metastasis to the brain, the internal surface of the aorta was highly reddened. Dr. Frank, of Vienna, observed the same thing in inflammatory fever; which form of fever, indeed, he considers as the result of arteritis. The appearances now mentioned, would seem to justify the application of the term general inflammation to pyrexia, or symptomatic fever, by Dr. George Fordyce. They may, however, be nothing more than the result of the long-continued violent action of the vessels in such cases; as this must naturally determine a greater flow of blood to them, with the further effect of increasing both their disposition to act, and their power of acting.

The disordered state of the sanguiferous system in pyrexia, as described above, by no means constitutes the whole of the affection. The disturbance in the condition of the brain is hardly less apparent than that observable in the organs of circulation. The affection of the brain, indeed, appears to constitute, if not the first, at least one of the most important links in the chain of morbid phenomena that characterise pyrexia, or the febrile state.

In the first, or cold stage, the affection of this organ is sufficiently obvious, from the disorded state of the sensorial functions. Thus, a sensation of cold is often experienced, while the body is, in fact, preternaturally warm. The sensibility of the surface too, is, at times, so much diminished, that the skin has been actually scorched by too near an approach to the fire, without the patient appearing to suffer pain. The voluntary muscles act independently of the will (rigor and tremor), and the mental power is always in some degree impaired.

The actual pathological condition of the brain, in this stage of the affection, can only be matter of conjecture. It is evidently a temporary state, and disappears with the paroxysm, or rather with the cold fit. From the known sympathy that subsists between the different parts of the capillary system, it is probable that the capillaries of the brain are in a state corresponding with those of the surface—that is, in a state of constriction. The paleness and cold-

ness of the face, the shrinking of the features, and the cord-like feeling that is usually complained of within the head in these cases, give countenance to this supposition. Such a state of the extreme vessels of the brain, (supposing it to exist,) and the consequent impediment it would necessarily create to the cerebral circulation, seem amply sufficient to account for the imperfect and disturbed state of the sensorial functions, as well as for the diminished energy of the whole system.

In the second stage, or hot fit, proofs, equally strong, of increased arterial action are discoverable in the brain, as in the rest of the system; witness the flushing and fulness of the face, the heat of head, and the violent and painful throbbing of the arteries, both within and without the skull. This increased arterial action in the brain is attended with more or less of excitement and disorder of the sensorial functions. Sleep is either deficient or disturbed; and the sensibility and irritability of both body and mind are in excess—a state altogether different from that which took place in the former stage.

Such appear to be the principal changes that take place in the system, in the different stages of pyrexia. The way in which they are produced by the ordinary cause, inflammation, is a matter involved in much obscurity. In regard to the mode of acting of causes in general, in the production of disease, little more than conjecture can be offered. The intermediate links that connect together cause and effect, are rarely submitted to our senses, and have always afforded a fruitful field for speculation and hypothesis. Enquiries of this sort, however, though they seldom lead to certain results, are yet not necessarily devoid of utility. They serve to open new views to the understanding, and suggest new trains of observation and experiment, which, now and then, lead to the discovery of important truths; while they serve, at the same time, to withdraw the mind from too implicit a reliance on received opinions; which, indeed, are often not less speculative and visionary than those for which they are unwillingly abandoned.

It will not be possible, I apprehend, to account for the production of pyrexia, or symptomatic fever, as a consequence of inflammation, without adverting to that most important connecting principle in the animal economy, the existence of which cannot be questioned, although the term by which it is usually expressed (namely, sympathy) has, by many, been objected to. It cannot be denied, that different organs influence one another in various ways; so that no considerable change in the condition of one part can take place without affecting others, in greater or less degree. This is, indeed, the proof of the connection or sympathy contended for; and it has not as yet, perhaps, been designated by a more appropriate appellation.

This sympathy, or consent, as it has been sometimes called, cannot always be accounted for, from any direct communication of nerves between the parts which thus influence one another; nor even from those parts deriving their nerves from a common origin

in the brain; for, in numerous instances of sympathy existing between organs, nothing of this kind can be traced.

The brain, nevertheless, appears to be essentially concerned in this connection. According to the experiments of Dr. Whytt, the motions which proceed from sympathy cease as soon as the brain is rendered unfit for action, or the communication with it interrupted; "because," as he says, "these motions depend upon a perception in that organ from which the nerves proceed, and where alone the cause of their sympathy is to be found.[1]

He observed, for example, that when the head of an animal was cut off, the stomach itself might be readily excited to increased peristaltic motion for a time, by irritating it; but actual vomiting could not be made to take place by the most powerful irritation; the sympathy between this organ and the diaphragm and abdominal muscles (the immediate agents in the act of vomiting) being destroyed. In like manner, in the human subject, in great injuries of the brain, and in many of its diseases, the iris does not contract from the impulse of strong light on the retina, because the sympathy between these parts no longer exists.

From these and other facts, it would seem that the brain is the great organ of sympathy, or medium of communication, between different parts of the body, as it evidently is between body and mind. And it is probably in this organ (the brain) that the first change takes place when inflammation is about to disorder the general system. The sufficiency of the changes in the state of vascular action and of circulation in the brain, alluded to above, to disturb the general vascular action throughout the system, as observed in the different stages of pyrexia, can hardly be questioned, when we advert to the great influence which the brain exerts over the actions of every part, and which is proved by the effect of various injuries and diseases of this organ.

Upon the whole, with regard to the intrinsic nature and mode of production of pyrexia, or symptomatic fever, it may, I think, be concluded that inflammation, considered as the ordinary remote cause of this affection, acts as a source of irritation to the brain, disturbing its vascular action, and thereby disordering its functions, but differently in the different stages. The change thus taking place in the condition of the brain, influences, subsequently, and in a corresponding manner, the whole sanguiferous system.

The affection of the brain, in this case, even in the hot stage, where the action of the cerebral arteries is manifestly and often violently increased, does not appear to amount to actual inflammation; yet it is readily convertible into this, where either the cause (namely, inflammation) exists with great intensity; or (which comes to nearly the same thing) where the disposition to inflammation in the brain happens to be unusually strong. When, for example, inflammation takes place with great violence, or to a great extent, in any part of the body; and more especially if it be seated in parts

[1] Whytt's Works, 4to. p. 518.

that are intimately associated with the brain, and which, on that account, exert a proportionately greater influence over it, (such as the different organs of sense, and very irritable structures in general,) inflammation in the brain is apt to ensue. In illustration of this, we may refer to erysipelas in particular, which, in ordinary and favourable circumstances, is attended only by slight pyrexia, or symptomatic fever; but when it occurs in habits that are predisposed, as it were, to cerebral inflammation, as in persons addicted to the use of intoxicating drinks, it is frequently followed by actual inflammation in the brain, and in that way often proves fatal.[1]

We have an example of a similar kind in small-pox. In mild cases of this disease, the primary or eruptive fever subsides, either wholly, or in a great measure, upon the appearance of the eruption. If this be trifling, in point of number, the febrile symptoms altogether disappear, and do not recur again; but if the eruption be very abundant, as in the confluent kind, the febrile state continues, till, at length, the great extent of the inflammation on the skin irritates the brain into actual inflammation; which, under the denomination of the secondary fever, often terminates in death.

There appears to be a considerable analogy, in many respects, between a febrile paroxysm, the result of inflammation, and the effects of the cold bath. In both, there is constriction of the capillaries, with tremor of the muscles, and smallness and irregularity of the pulse. These are followed, in both cases, by reaction, or a hot fit. I know not how otherwise to explain this, than by stating the fact, that most great changes that occur in the system, and especially when they take place suddenly, are, in like manner, ushered in by shivering, or a kind of cold fit; and this succeeded by a hot one. This may be observed on the approach of suppuration, and of parturition, on the passing of gall-stones, and the introduction of the catheter.

It seems not unreasonable to believe, that, in all these cases, the series of changes begins in the brain, and consists in constriction of the capillaries there; and that this is followed by a similar constriction of the capillaries generally, throughout the body. This appears the more probable, when it is considered that the same effect is produced by certain mental emotions, such as terror or disgust; where the change evidently commences in the brain. Another argument in favour of the supposition that the series of changes here alluded to commences in the brain, may be derived from the fact, that, when this order is rendered incapable of performing its functions by the use of narcotics, the body becomes insensible to the application of cold, however intense. And in such

[1] Those who have once suffered severely from inflammation of the brain, are apt to have the inflammation renewed by the occurrence of even slight disease in other organs. A young lady who, when an infant, was the subject of inflammation of the brain, in the form of acute hydrocephalus, was seldom afterwards attacked by catarrh, without delirium, and other marks of cerebral inflammation, supervening. I have observed the same in many other instances.

cases, there is neither constriction of the capillaries, nor involuntary tremors of the muscles, produced by it.

From all that has been now said, it may be concluded, that very generally, if not universally, pyrexia, or a febrile state of system, is the consequence of inflammation; the existence of which, therefore, it so far serves to prove. The practical inference to be drawn from the admission of this, as a principle, is of no small importance. Knowing that, wherever febrile symptoms appear, inflammation is going on in some part of the body, we endeavour to discover its seat, and which may generally be done, either by enquiring into the feelings of the patient, (most inflammations being attended with more or less of pain,) or by examining successively the different functions; which inflammation, when present, is sure to disorder, in greater or less degree. The advantage of this is obvious. Our efforts to subdue inflammation are often more successful, when directed to the immediate seat of the disease, than when administered, as it were, at random; for the treatment is always more or less modified by the nature of the part affected. In many instances, no doubt, inflammation is more beneficially influenced by general than by local treatment; and often the part is so situated, as to be inaccessible to direct remedies. Still, there is much advantage in being acquainted with the organ primarily affected, in order that attention may be paid to the exercise of its functions. The brain furnishes a good example of this; for in phrenitis, fever, or any other active disease of this organ, there is nothing more injurious than the undue exercise of either sensation, voluntary motion, or mind.

SECTION VI.

OF THE CONSEQUENCES OF PYREXIA.

Pyrexia, by the violent vascular action that attends it, has the effect, like all other excessive actions, of expending too rapidly the vital power, and thus becomes a cause of general weakness to the system. Another never-failing consequence of long-continued febrile action, is general emaciation. This effect is always in proportion to the violence and duration of the febrile symptoms altogether. Thus, in all acute inflammations, accompanied, as they usually are, by much febrile action, the body wastes rapidly. So after ordinary continued fevers, of only a few weeks' duration, both the weight and bulk of the body are found to be greatly diminished; and in hectic fever, protracted, as it often is, to a period of many months' duration, the emaciation of the body is at length extreme. This effect, in the cases here adduced, is to be ascribed partly to the general disorder of functions (especially the nutritive) by which the regular supply of the body is interrupted; but in part, also, to the increased activity of the absorbents, without which it is

not conceivable that such a waste of both solids and fluids should take place.

Pyrexia has likewise a tendency to produce hemorrhage; and, where a predisposition to hemorrhage already exists, it often has this effect. It is occasionally also followed by anasarca, as is observed after ordinary fevers of protracted duration; and, indeed, after long-continued febrile action of any kind. This effect is attributable to a loss of balance between the processes of exhalation and absorption. Both are probably in excess; but the former predominates.

A still more important consequence of pyrexia is its generating a disposition to inflammation throughout the system, and which often shows itself in the most important organs. This is owing, no doubt, to the excited state of arterial action that makes so prominent a part of this affection. Nothing, for example, is more common in the course of simple idiopathic fever, than for a secondary inflammation to arise in some part, in addition to that already existing in the brain, which constitutes the essential part of the disease. In hot climates, the abdominal viscera are most apt to be thus affected; in cold ones, those of the chest. In this way, simple fever becomes complicated in its characters, and the danger is thereby often greatly enhanced. In intermittents of long continuance, the frequent recurrence of the febrile paroxysms seldom fails to give rise to obstructions, as they are termed—that is, to chronic inflammation—in some of the viscera, particularly in the liver and spleen; but occasionally, also, in the lungs. Such combinations always render the treatment more difficult.

In like manner, in acute rheumatism, a disease in which the febrile symptoms run higher than in any other disease, the disposition to inflammation thus generated throughout the system is proportionally strong; the brain, or lungs, or heart, becoming thus affected, a quickly fatal termination is frequently the result. Chronic diseases of the heart, also, attended with disorganisation in various degrees, and clearly the result of continued inflammation, are traceable, in numerous instances, to acute rheumatism, as their source.

In various other long-protracted and incurable diseases, that are accompanied with chronic pyrexia, or hectic fever—such as cancer, diseased joints, and others—slow inflammation often arises in the lungs, and creeps on, almost imperceptibly; producing disorganisation (tubercles), and ending in fatal pulmonary consumption; which is, indeed, the immediate cause of death in most of these cases.

Phthisis pulmonalis itself, when a primary affection, and attended as it generally is, by long-continued febrile action (hectic fever), seldom fails to be accompanied, towards the close of the disease, by inflammation of the mucous membrane lining the alimentary canal; at first, in the form of diarrhœa, and, at last, of aphthæ, or thrush, in the mouth and fauces. Frequently, also, boils appear on the skin; and (if we may judge from the delirium that often occurs

towards the close of life, in consumptive patients) the brain itself appears to suffer inflammation in a slight degree.

The dependence of all these secondary inflammations on pyrexia, as their cause, appears from hence, that they are always the more conspicuous, the more active the febrile symptoms are observed to be.

SECTION VII.

OF THE TREATMENT OF PYREXIA, OR SYMPTOMATIC FEVER, IN GENERAL.

In considering diseases that are attended by a multiplicity of symptoms, varying in the time and order of their occurrence, it is necessary to distinguish those symptoms that are primary and essential (pathognomonic symptoms, as they are called) from such as are termed secondary (as arising out of the former); and likewise from such as are accidental, or of occasional occurrence only. These distinctions are of importance as respects diagnosis; but they are still more so in regard to practice. The relief of the secondary or accidental symptoms of a disease is seldom more than palliative; and it not unfrequently happens, that the means employed for the purpose of alleviating particular symptoms are adverse to the primary disease. Of this, it were easy to adduce numerous examples. For instance, the giving opium merely to relieve pain, or to procure sleep, in cases where the pain and watchfulness are the result of active inflammation; the suddenly checking diarrhœa occurring in the course of fever, by opiates and astringents; and the suppressing a cough, in recent catarrhal or pulmonic inflammation, by similar means, where a free expectoration is, perhaps, the thing needed; in all these cases, the primary disease is often aggravated by such merely palliative measures.

These remarks apply to pyrexia, which is always a secondary affection, as before shown. In this case, however, as in others, there are numerous occasions in which secondary symptoms require to be attended to, and even demand our principal attention in practice. In the first place, because they are sometimes more painful, and more dangerous in their consequences, than the primary disease itself; and next, because, in many cases, we can only influence the primary disease through the medium of the secondary symptoms. To understand this, it is necessary to advert to the influence which pyrexia is capable of exerting on the primary disease.

Pyrexia sometimes appears to react on the primary disease, so as to increase its violence. This may be observed in the case of acute rheumatism, as well as other active inflammations, when they occur in vigorous subjects; the local pains are generally in proportion to the severity of the febrile symptoms; these, therefore, it often becomes necessary to moderate. But in slight and recent cases of inflammation, taking place in weak and delicate subjects,

the occurrence of pyrexia sometimes appears to conduce to the removal of the primary disease, and should, therefore, be rather encouraged than suppressed. I may cite the following as examples of this :—

Instances occur, almost daily, of slight inflammations, occasioned by cold, (as catarrh, sore throat, rheumatism, &c.,) being successfully treated by stimulants of an active kind, under the denomination of diaphoretics or sudorifics; such as camphor, ammonia, the hot bath, and various others. These, by exciting the sanguiferous system, rather tend, at first, to produce or aggravate the febrile state; but if they be so managed as to induce sweating, the febrile symptoms often subside, and, at the same time, the primary inflammation which caused them. This mode of practice, however, is rather equivocal in its effects, and requires caution; for where it does not succeed in removing the primary inflammation, it is sometimes found to aggravate it. Whenever, therefore, the inflammation is seated in organs of importance to life, and which it is desirable to suppress quickly, it is hardly advisable to proceed in this way. The sudorific practice, however, is rendered more safe in such cases, and, at the same time, more effectual, by previous abstraction of blood, in proportion to the habit of the patient, and other circumstances.

In ordinary continued fevers, of a mild description, this mode of treatment (the diaphoretic or sudorific) has been, and indeed still is, in very general use, in preference to the more active, though unquestionably more effective, means of cure, by blood-letting. The ground of preference here seems to have been the preservation of the general strength. This object, however, is more frequently frustrated than promoted by such practice. This mode of treating fever fails to have the desired effect (that of speedily putting a stop to the disease) in the greater number of instances; and the result then usually is a long-protracted disease, with great exhaustion of the vital power, and general emaciation; with the further risk of lasting, if not fatal, injury being done to the structure of the organ primarily affected, by the long continuance of inflammation in the part. It is almost invariably found, in cases of fever, that the lost strength is more rapidly recruited, and the cure far more complete, when the disease is treated actively in the beginning, (as by blood-letting and other simple antiphlogistic means,) than when the fever is allowed to run its ordinary course, under the use of what are miscalled milder means, but which, in the end, are infinitely more distressing to the patient. It must be borne in mind, however, that blood-letting is limited almost entirely to the earliest stage of the disease; for when a fever has gone on uninterruptedly for several days, it is seldom within the power of art to arrest its progress. The disease must then be allowed to pursue its course (the object being merely to palliate particular symptoms), till it arrives at its natural termination. As this termination approaches, and when the violence of arterial action has considerably subsided, as judged of by the pulse, the moderate employment of

stimulants, in the form of wine, &c., is attended with real advantage.

In violent or malignant fevers, as they are called, which are now happily of rare occurrence, it was formerly the practice to induce profuse sweating by the use of the most active stimulants—such as combinations of alcohol, spices, camphor, ammonia, and others. Instead of the cool and simple regimen now in general use, and upon which the comparative mildness of fever at present chiefly turns, the patient was made to breathe a heated and generally an impure atmosphere, which, with the stimulant remedies above mentioned, actually produced the so-called train of typhoid or malignant symptoms they were designed to obviate. Sydenham himself was led, in many instances, to adopt the sudorific practice in the treatment of fever; not, however, from considering it the most useful, but because, as he says, he found it often difficult, from the prejudices of the age, to carry blood-letting (which he found to be by far the most useful and effective) to the required extent; and, unless this were done, the remedy appeared to be rather injurious than beneficial. The stimulant or sudorific practice above alluded to must, doubtless, have succeeded in many instances, or it would not so long have maintained its ground, as was actually the case. It is certain, however, that the motive for employing it—that of carrying noxious matters out of the system—was altogether hypothetical and inconsistent with sound pathology; and it is not less certain, that the success attending the practice was far from being such as to make it an object deserving of our imitation.

In fevers of the intermitting kind (agues), before the introduction of the cinchona or Peruvian bark, it was the general practice to excite the sanguiferous system during the paroxysm by the most powerful stimulants—in other words, to increase the febrile action. By the exhibition of such remedies, and the profuse sweating that usually followed, a more perfect *crisis* was sometimes obtained, and the further recurrence of the paroxysms prevented. This mode of treating intermittents is now rarely practised, a preference being justly given to the cinchona and remedies of the same description; the former, the stimulant plan, being always uncertain in its effect, and, when it fails to cure, calculated rather to aggravate the disease: intermittents being thus often converted into continued fevers, while continued fevers are rendered more dangerous by every unsuccessful attempt of this sort.

The specific or eruptive fevers, especially small-pox, were for a long time treated in the same manner, but with the worst possible effect; and the practice is now very properly abandoned. In the very advanced stage of these, as of other fevers, as well as of inflammation in general, when the vascular action is failing, as shown by feebleness of the pulse, one feels a propensity that seems natural to resort to wine and other stimulants; and if they are used with great moderation, they really appear to be beneficial—not acting, however, upon the principle of *supporting the system*, (a mode of expression that appears to me to involve an absurdity,) but, by a

kind of counter-irritation, relieving the local disease by exciting a more general action throughout the system, and thus equalising the distribution of the vital power.

With the exceptions now mentioned, it is generally proper to endeavour to subdue, or at least to mitigate, pyrexia, or the febrile symptoms that attend inflammation; although the doing so may have no material influence over the primary disease.

In violent inflammations in general, wherever seated, and especially if the patient be of a vigorous or inflammatory habit, and the disease recent—attended, too, as it commonly is under such circumstances, by high febrile action—it is essential to endeavour to subdue this by the most active means; not only for the purpose of guarding against the consequences formerly mentioned, but also as the best means of influencing the primary disease itself: for in the most important inflammations, the part primarily affected is out of the reach of direct or topical remedies. And even where it is accessible, experience shows that local treatment is often less efficacious than the use of general remedies. Accordingly, it is found that the reduction of the febrile action, by bloodletting and other antiphlogistic means, proves the first, and often indeed the only, step requisite to the cure—the primary disease giving way immediately that this object is attained. It is, however, important to repeat here, that this is not universally the case; for the febrile symptoms may wholly disappear, while the primary inflammation still continues, though perhaps in a mitigated form. Our care, therefore, is not to be limited merely to the removal of the febrile symptoms in inflammations of important organs—such as the brain or lungs—but should be continued till the local signs, as well as the general disorder of system, have wholly disappeared. The neglect of this, as a rule of practice, often leads to fatal mischief at a remote period afterwards; as in pneumonia more especially, where, if the slightest degree of inflammation be allowed to remain in the lungs, it is likely to produce disorganisation, the ultimate result of which often is pulmonary consumption.

In hectic fever (chronic pyrexia), attending ulceration or other change of structure in the lungs, the reduction of the febrile symptoms appears to be the chief, if not the sole, means of arresting the progress of the disease; for I scarcely need observe, that all the boasted *specifics* that are daily and confidently obtruded on public notice, as cures for consumption, are wholly undeserving of attention, their utility being disproved by abundant and fatal experience. By acting upon the simple principle of lessening febrile action, we do all the good that the case admits of: we relieve the most distressing symptoms of the disease, and, by so doing, prolong life. And further, should a cure be attainable, (which undoubtedly is sometimes the case,) it is only to be accomplished, as far as I have observed, by acting upon the same principle.

In ordinary fevers of a mild description, when the time for active treatment is past, so that no reasonable expectation can be entertained of interrupting at once the further progress of the disease, a

favourable termination mainly depends upon keeping the febrile symptoms within moderate bounds. For such a purpose, however, the mildest antiphlogistic treatment only is required; such as the admission of cool and fresh air, cooling drinks, and mild aperients from time to time. The neglect of these, and the substitution of stimulant means (upon the absurd plea of supporting the strength), is a frequent cause of the disease assuming what is called a typhoid form in its latter stages.

In small-pox, and in the specific fevers in general, (for which we can hardly be said to possess any absolute remedy,) our chief attention should be directed, and indeed is limited, to the single purpose of moderating the febrile symptoms; by doing which, the danger is in a great degree obviated, while the disease passes mildly and quietly through its course.

SECTION VIII.

OF THE PARTICULAR MEANS APPLICABLE TO THE RELIEF OF PYREXIA.

Increased heat being the most obvious, as well as one of the most constant, of the symptoms termed febrile, the reduction of this becomes a principal object of attention in practice. The different means applicable to the purpose have been termed *cooling*, *refrigerant*, or *antiphlogistic*—all terms of similar import. They have been also called *sedatives*, as allaying vascular action, upon the excess of which a morbid evolution of heat in animal bodies chiefly depends.

The means are various, and adapted to different circumstances. They consist principally of the following, and may be thus arranged, according to their degree of efficiency:—1, bloodletting; 2, other evacuants—as purgatives, emetics, sudorifics, and blisters, (all of which, however, are far from acting on the same principle); 3, foxglove, and perhaps others of the narcotic tribe; 4, preparations of lead; 5, cold, applied in various ways; and, 6thly, acids and different neutral salts.

1. Of bloodletting, as a remedy for pyrexia.

In the employment of this potent remedy, it is not only necessary to consider its power of subduing febrile action simply, but also the influence it may exert over the primary disease. Nor must the condition of the general system at the time be overlooked. Without a due consideration of all these points, indeed, the remedy can neither be properly nor even safely resorted to.

For the simple purpose of mitigating or subduing febrile action, bloodletting is only called for where that action is in great excess, so as to become of itself, either directly or indirectly, a source of danger. When used with such an object, it requires only to be carried to a moderate extent—as from eight or ten to perhaps six-

teen ounces, according to the age, habit, and other circumstances, of the patient. In the case of violent and recent inflammation, occurring in vigorous subjects—accompanied, as such cases almost necessarily are, by a high state of febrile action—bloodletting cannot safely be dispensed with, and particularly when organs important to life are concerned.

A very general aversion from bloodletting, however, appears to prevail, when the patient is weak, even although considerable pyrexia should be present, as is often the case. This objection, according to my observation, is not in general well founded. A febrile state of body argues the existence of inflammation, equally in the weak as in the strong. The intrinsic nature of the disease is the same, and the treatment is governed by the same general principles. It is only necessary that the remedy should be properly adjusted to the existing circumstances; and, with this limitation, bloodletting will be found both as safe, and as effectual, in the one case as in the other.

The case of hectic fever, as occurring in pulmonary consumption, serves to illustrate what has now been said. In the confirmed state of this disease, a cure is rarely to be expected from this or any other means. Yet much may be done to mitigate the violence of febrile action (the hectic); by doing which, much suffering is spared to the patient, and life often prolonged to a considerable extent. These important objects may be accomplished in many instances by bloodletting, repeated from time to time, but always in small quantities, as from three or four to five or six ounces at a time; the rule being, in such cases, so to draw blood, as not sensibly to disturb the functions or feelings of the patient. When used with these precautions, and while a tolerable share of general strength still remains, the good effects of small and repeated bleedings are often strikingly displayed, by their relieving all the most distressing feelings of the patient, inducing quiet sleep, and restoring appetite if lost. And I may further add, that if the disease admits of a cure, as undoubtedly is sometimes though rarely the case, it is most likely to be accomplished by such means; and this I have seen in different instances. I consider it, however, to be essential to the success of this mode of treating pulmonary consumption, whether undertaken with a palliative or curative intention, that the patient should be allowed to take food, either animal or vegetable, as his habits and inclinations may lead him. And it is no small recommendation of this practice, that not only is appetite usually excited by such small bleedings, but animal food may then be taken without producing that feverish excitement that it is otherwise apt to do.

Venesection is greatly preferable to other modes of drawing blood for the relief of pyrexia; because this being an affection of the sanguiferous system chiefly, is most influenced by that mode of taking away blood that has the speediest and most general influence over the circulation.

I before observed, that, in the employment of bloodletting as a

remedy for pyrexia, it is not only necessary to consider its power of subduing febrile action, but also to take into account the nature and circumstances of the primary inflammation, for this is not always, nor necessarily, benefited by that remedy. Many inflammations have a strong natural tendency to subside spontaneously, after running through a certain course. The most frequent example of this is furnished by inflammation of the mucous membrane, whether of the respiratory or alimentary passages, or any other. The natural tendency of inflammation in this structure is, to terminate in an increased secretion of mucus, and which, after a while, gradually declines, leaving the structure and functions of the part unchanged. Now bloodletting in these cases, if it be carried so far as to weaken considerably the general system, or to disorder materially its functions, disturbs the natural course of the disease, so as often to prolong its duration, and render the cure more difficult. It is only, therefore, where the febrile symptoms run unusually high in such cases, that bloodletting is required for their mitigation.

I may observe here, however, that there is no foundation for a notion that has prevailed of late (and only of late), namely, that there is something so peculiarly depressing in the nature of inflammation, when seated in the mucous membrane, as to preclude the use of bloodletting altogether. Instead of this being the case, I venture to assert that, when called for by the severity of the symptoms, bloodletting is as useful, and indeed as necessary, in the treatment of catarrh, diarrhœa, and other inflammations of the mucous membrane (provided they be recent), as in that of any other structure.

Various others of the class of evacuants are powerful in diminishing pyrexia or febrile action, though, generally speaking, they act upon a very different principle from that of carrying matters out of the system. They seem rather to act upon the principle of counter-irritation; as, indeed, bloodletting itself probably does in a great measure. And, upon the same principle, they not only tend to relieve pyrexia, as the effect of inflammation, but often, also, the inflammation itself.

Formerly, sudorifics were most relied upon, and were universally resorted to, in febrile disorders of all kinds; and, doubtless, very often with good effect. The comparative advantages and disadvantages of this practice have been already pointed out.

Emetics are powerful in breaking the chain of morbid phenomena that constitute the febrile state; and, in so doing, they certainly act, for the most part, upon the principle of counter-irritation. Exhibited at the outset of ordinary idiopathic fever, and repeated for several times, at intervals of a few hours, I have often observed them to interrupt the perfect formation of the disease, so as to bring it to a speedy termination. Emetics have been used likewise with advantage in intermittents; as, doubtless, they might be for the relief of various other febrile disorders.

Much has been said and written of late in favour of the employ-

ment of large doses of the tartarised antimony; not, however, on account of its emetic properties, nor as a sudorific; but as a direct sedative, for lessening febrile and inflammatory action. The earliest account of this practice that has come under my observation is contained in a work on the "Petechial Fever of Geneva," as it occurred in the years 1808 and 1809; published at Milan, by Dr. G. Rasori, and noticed in the Medical and Physical Journal, for February 1820. Dr. R. is an advocate for the inflammatory nature of fever in general, even including the petechial variety; and he advises the use of antiphlogistic remedies accordingly. The chief of those which were successfully employed by him in the fever in question, were antimonials and saline purgatives, especially the supertartrate of potass. He considers antimony as a direct sedative, or counter-stimulant; independent of any evacuation, or other sensible operation. Opium, wine, and all the other usual stimulants, were found to be hurtful, even when what are called nervous symptoms occurred, as subsultus tendinum, and the like.

The practice of giving the tartarised antimony in large doses, has, as usual, found imitators in this country; and some have even gone so far as to assert, that it is an adequate substitute for bloodletting in the treatment of inflammation in general. Judging, however, from my own experience on the subject, after numerous trials; aware also of the fallaciousness of what is generally called experience in physic, I confess I am inclined to be skeptical with regard to this, as well as various other points, that appear to be taken for granted by some in the present day. The credulity displayed of late by the public, and indeed by many members of the medical profession, regarding the supposed efficacy of the Hahnemanic or homœopathic practice, is well calculated to lessen one's confidence in all such extraordinary statements.

Purgatives have been in great vogue, of late years, as anti-febrile remedies; while emetics have been comparatively neglected. In the preference given to the one or the other of these, however, whim or fashion, probably, rather than any well-grounded experience, has determined the choice; for in medicine, as in most other affairs of life, fashion has a paramount influence.

Among the remedies calculated to subdue febrile action, the foxglove (digitalis purpurea) merits particular notice. This plant has a surprising, it might, indeed, be said, a specific, power, of lessening the frequency of the pulse; thereby diminishing animal heat; and thus febrile action is relieved. This remedy is an important auxiliary to bloodletting in febrile disorders in general; and is likewise applicable in many cases, where the latter is objectionable, on account of its debilitating effect. The action of the digitalis is, for the most part, temporary, and of short duration; while it does not diminish the vital power, as bloodletting necessarily does when carried to any considerable length. It is as a sedative in the hectic fever that attends pulmonary consumption, however, that the value of this drug is the most strikingly displayed. It commonly diminishes, and sometimes subdues altogether, the febrile symptoms.

Under its beneficial operation the night-sweats disappear, the sleep becomes calm and refreshing, and the appetite is restored. The progress of the disease towards its usual fatal termination is often arrested for a time; and, more than this, a cure is sometimes thus effected, especially when the circumstances admit of its being used in combination with small and repeated bleedings, as already mentioned. A great drawback from the usefulness of the foxglove is, the uncertainty as well as the occasional violence of its action. It will not at all times, howevever administered, reduce the frequency of the pulse; and when this is the case, it fails to produce its beneficial effects.

Opium, by its influence on the brain, sometimes lessens the irritability of the heart, so as to render its action slower, and that even in cases where the digitalis fails to have this effect. By so doing, it may lessen febrile action. Its effect in this way, however, is uncertain and equivocal. It is only in cases of great irritability of the system altogether, where the pulse is exceedingly frequent, and does not become slower by bloodletting or the digitalis, that opium, as a sedative, appears to be admissible. In the generality of cases of inflammation, opium is found to aggravate rather than diminish febrile action; and it has the further effect of checking the secretions, upon the freedom of which the cure often depends. Opium has also the disadvantage of masking the symptoms, by rendering the patient insensible to pain for a time, and which it may do without at all relieving the primary disease. Whether the other narcotics have the effect of lessening febrile action, I am not prepared to state. Tobacco, however, appears to exert a sedative influence over the sanguiferous system, and might, perhaps, be usefully employed for such a purpose.

It was remarked by Dr. Whytt,[1], that wine, and even brandy, under certain circumstances, act as sedatives to the sanguiferous system, cooling the body when preternaturally heated. This effect is to be ascribed, probably, to the action of these substances on the brain; diminishing, like opium, the irritability of the whole system, and of the heart and blood-vessels in particular: whence the pulse becoming less frequent, a diminished evolution of animal heat is the result. We have a good example of what is now stated in the latter stages of fever; in which a moderate quantity of wine is often found to lessen the frequency of the pulse, and, at the same time, to increase its strength and fulness.

The preparations of lead have undoubtedly a powerfully sedative influence over the sanguiferous system, so as, on many occasions, to lessen febrile action. The saccharum saturni (*plumbi acetas*), the only one in use, has frequently been employed for the purpose of diminishing the night-sweats in pulmonary consumption; and it was even supposed capable of curing the disease altogether. Accordingly, a solution of this substance was long kept in the shops, under the name of the anti-phthisical tincture. The known

[1] Whytt's Works, 4to. p. 647.

deleterious properties of this metal, however, have prevented its general employment. It may nevertheless be both usefully and safely employed (where other means fail) for the relief of hectic fever; but it should be used with much caution, and for a limited time only. It has been occasionally, though rarely, used in ordinary continued fever; with what effect I am not qualified to speak.

It is customary to employ acids, and likewise some of the neutral salts, as refrigerant or anti-febrile remedies; such as common nitre, the acetate of ammonia, and the citrate of potass (the common saline draught). These serve well enough to amuse patients, and fill up the routine of ordinary practice; but they are of little real efficacy.

Cold, properly applied, is a real sedative, and powerful in allaying febrile action. In ordinary fevers, it serves to mitigate the symptoms altogether, and contributes much towards a favourable termination of the disease. In fevers of the specific kind, especially the small-pox, it is of still greater importance, and conduces, more than any other circumstance, perhaps, to the safety of the patient. In all cases, where cold is resorted to for the purpose of moderating febrile action, it should be applied steadily, and so as to occasion as little disturbance as possible in the system. The admission of cool air to the surface—cold drinks—and occasionally spunging of the body, either partially or generally, with cold or even tepid water, is, in general, all that is required.

It seems scarcely necessary to observe, that the regimen of the patient should correspond with the medical treatment in cases of pyrexia. On many occasions, the regulation of the food and drink, air, exercise, &c., constitutes all that is essential. Numerous instances are on record of chronic inflammations, when accompanied with a febrile state of body, yielding to long-continued abstinence, and that where a more active treatment has failed. Pulmonary consumption appears to have given way, in many instances, to perseverance in a milk and vegetable diet; and I think I have seen cancer arrested in its course, or, at all events, greatly mitigated in its symptoms, by the adoption of a vegetable diet, as suggested by Dr. Lambe.

Increased circulation constituting an essential part of the character of pyrexia, it is generally proper to abstain from much bodily exercise, from the tendency it has to increase the action of the sanguiferous system.

Since the foregoing pages were written, my attention has been called to a work, recently published, under the title of "The Elements of Medicine," by Drs. Bright and Addison, Physicians to Guy's Hospital—a work that cannot fail to be read with much interest, as well on account of the simple and unostentatious style in which it is composed, as from the abundant proof it affords of the great practical knowledge of the writers, which has qualified them, in no ordinary degree, for the task they have undertaken.

The subject of fever naturally engages a considerable share of their attention ; and I have been much gratified at finding no small degree of similarity in our opinions regarding it. I by no means intend to say, that these gentlemen go the length of adopting, in direct terms, and to the fullest extent, the principles I have advocated; yet a remarkable coincidence will not fail, I think, to be observed ; as may be seen by comparing together the conclusions we have respectively arrived at, and the facts from which they are deduced.

The theory of fever which I have endeavoured to maintain embraces two distinct points, which require to be separately considered —the seat and the nature of the disease.

My first position is, that fever, strictly so called, or idiopathic fever, as it is more commonly termed, is not a general but a local disease, the seat of which is the brain or encephalon—and, judging from the great diversity of the symptoms, the disease would appear to occupy the entire organ ; though unequally, and differently at different times and in different individuals. Hence it is, that certain of the sensorial functions are sometimes more disturbed than others, though all are so in greater or less degree. In one individual, for example, there is observed an intolerance of light—in another of sound ; and so of the other external senses. In some, the voluntary power is more depressed than in others. In some, there is early delirium ; while in others, the mental power is simply impaired.

The same inequality may be observed in regard to the secondary functions of the brain (if the expression may be allowed)—I mean, those functions, the organs of which are remotely situated, and which, though deriving much of their energy directly from the brain, and therefore liable to be disordered by it, are yet, to a certain extent, independent of it : it will be sufficient to mention here the stomach and sanguiferous system, which belong rather to the ganglionic or organic, than to the cerebro-spinal or animal nervous system. Thus, it is found that, although the functions of the stomach are always impaired, and, indeed, for the most part suspended, in fever, there is great difference observed in regard to the extent to which this organ is disturbed at different times. On some occasions, vomiting forms a prominent character of the disease, while it is not met with at all in others. So in most fevers, the action of the heart is much quickened, as shown by the pulse ; in some, however, the reverse takes place, the pulse becoming preternaturally slow : and there are not a few in which the pulse is little altered from the natural state. In some peculiar instances of fever, the capillary extremities of the arterial system appear to be in nearly a paralysed state ; losing their contractility, and allowing the blood to escape into the interstices of the skin, and from the different surfaces and excretories—the malignant and spotted fevers, so called.

The disorder of the cerebral or sensorial functions above described, and which is so constantly observed in cases of proper or

idiopathic fever, constitute, in fact, the pathognomonic or distinguishing character of the disease; for the mere febrile symptoms—the hot skin, furred tongue, and accelerated pulse, are common to all inflammations, and indicate nothing in regard to the particular seat of the disease.

Now, if we compare with this the description of fever in general, as given by the authors of the "Elements of Medicine,"[1] they will be found to be essentially alike; differing little more than in terms, and in regard to the part in which the symptoms are supposed to originate; the seat of disease in the one case being referred to the brain exclusively, while in the other it is referred to the nervous system altogether.

1st. "In every case of fever," the authors observe, "the causes producing it inflict a morbid impression upon the nervous system, by which the functions of that system, intellectual and bodily, are deranged, that derangement differing in degree in different cases: and, in consequence of this derangement, every organ, and every function of the body, appears to be more or less disordered."

2dly. "In every idiopathic fever, this morbid condition of the nervous system is associated with, or presently succeeded by, a deranged or excited state of the circulation; this derangement of the circulation differing in degree in different cases, and displaying a greater or less tendency to congestion, or even inflammation, of particular parts:" and,

3dly. "In the progress of every idiopathic fever, the secretions or excretions of the body become deficient, vitiated, or even irritating to the parts with which they come in contact."

In this description it is evident that the authors have fallen into the common error (as I conceive it to be), of confounding the primary and essential symptoms of the disease with those that are secondary, and common to all febrile disorders. It is under the first head only that the pathognomonic or distinctive characters of fever are to be found; and which consist, as here stated, in a derangement, in different degrees, of the functions of the nervous system, or as I should say, of the brain exclusively. The deranged or excited state of circulation, and (the natural consequence of this) the disordered state of the different secretions, come under the head of pyrexia or symptomatic fever, such as results from inflammation in general, and by no means belong to proper or idiopathic fever peculiarly, as I trust has been already sufficiently shown.

In developing the character of idiopathic fever, the authors follow the example of Dr. Cullen, in dividing the disease into three species, namely, the intermitting, remitting, and continued; and, like him, considering the intermittent as the type that best serves to illustrate the whole genus. To this, however, there are weighty objections. The intermittent (and likewise the remittent, for this is merely an aggravated state of the former) may be considered rather as a specific disease; peculiar in its characters, in its cause,

[1] Elements of Medicine, p. 5.

and also in its treatment; in all which respects it differs from the continued form of fever, which it scarcely more resembles, indeed, than does the small-pox, or any other of the fevers of acknowledged specific origin. It is to the continued form of fever, therefore, that we must look, in order to find the distinctive characters of fever altogether.

Continued fever is arranged by the authors under the three heads of mild, severe, and complicated; the two former differing only in degree; the latter marked by a predominant affection of some particular organ, but especially the alimentary canal, brain, or lungs. This secondary affection rendering the fever complicated, the authors (in deference, it would seem, to general opinion, rather than their own convictions) hesitate to refer to actual inflammation of the organs so affected; yet, if the description here given of such complications of fever be carefully perused, it seems to me impossible to come to any other conclusion, than that inflammation is actually present in those organs. Still, these are but accidental combinations, and do not at all decide the question as to the intrinsic nature of fever. It is the simple and uncombined form of the disease, whether mild or severe, that must be looked to for such a purpose; for in this only can the distinctive characters be found.

The authors describe the mild form of fever as it usually appears in children and in adults respectively. After enumerating the ordinary febrile symptoms that are common to all, they remark that "the principal sources of complaint are the headache and pain in the limbs. The headache, however, although it may occasionally suffer some aggravation towards night, is very rarely attended with any intellectual disturbance beyond a slight confusion on first awaking from sleep, which, even in this mild form of the disease, is for the most part disturbed and unrefreshing." In adults, the symptoms, though still mild, are described as somewhat more severe: "The headache, the depression of strength and spirits, the restlessness and want of sleep, are all found to take place in a higher degree, with, in some instances, slight accessions of delirium during the night: and to these is added, an expression of distress in the countenance that is highly characteristic of continued fever." The duration of the disease in adults is apt also to be more protracted; while there is, at the same time, a strongly marked tendency to assume a more severe character.

Such are the symptoms that are said to characterise fever in its simplest and mildest form, and which cannot, I think, be otherwise considered than as the result of a disturbed state of the sensorial or proper functions of the brain. The severer form of the disease, as here described, points still more strongly to the brain as the primary seat of the disease. The aggravated characters sometimes appear at the outset of the disease; sometimes they arise during its course, and that either without any assignable cause, or from injurious treatment or bad management. The accession of bad symptoms during the course of a previously mild disease is thus forcibly depicted:—"The languor and debility become more apparent; the

eyes appear dull ; the countenance more shrunk, and expressive of distress ; the breathing becomes somewhat laborious, and the voice more feeble ; the tongue, from being moist, gets dry, and partially covered with a brownish fur ; the skin is parched ; the pulse more frequent and more compressible ; and the patient sinks into a more supine position. The symptoms are speedily succeeded, or actually accompanied, by a more manifest disturbance of the brain. This disturbance, at first, perhaps, consists merely in a slight degree of confusion on awaking from sleep ; the confusion, however, soon amounts to actual wandering, especially during the night, but sometimes, in many instances, throughout the whole of the day. In other instances, the cerebral substance shows itself in the form of confusion or stupor from which the patient is with some difficulty roused, and into which he presently again relapses. If we do rouse him, his answers to questions are short, and for the most part incoherent ; and he will often continue muttering to himself, although no one is near. It not unfrequently happens that, with a greater or less degree of this latter modification of cerebral disturbance, the patient will manifest more or less of nervous agitation, almost amounting, in some instances, to tremor, especially when any one addresses him, or on attempting to make any exertion. The tongue is sometimes morbidly red and clean, or polished ; at other times dry, and of a yellowish-brown colour ; and it will tremble when protruded. This state is also attended with remarkable drowsiness, and repeated flushings of one or both cheeks."—" When such cases prove fatal, the oppressson of the several organs increases ; the exhaustion of the vital powers becomes extreme ; the stools and urine pass off involuntarily ; and the patient lies supine and helpless in bed, muttering and insensible : whilst, in the worst cases, hemorrhages of dark blood take place from the nose, from the bowels, or by urine ; or blood is poured out by the cutaneous vessels, producing spots of a livid or purple colour, or marks resembling bruises ; in which wretched plight the patient lingers, till death terminates his sufferings."

Such is the striking picture of the aggravated state of fever here drawn ; and it appears to me next to impossible not to refer such symptoms to the sensorium—in other words, to the brain ; for they consist entirely in a disturbance of the proper functions of this organ—the sensorial. The authors, indeed, refer to the nervous system altogether as the seat of the disease, rather than to the brain exclusively. But that an affection of the brain alone is adequate to the production of such symptoms seems sufficiently proved, both by the effect of injuries and acknowledged diseases of this organ. And if this be true, it is hardly consistent with just reasoning to go further, and multiply causes where one alone is sufficient for the purpose of explanation. Indeed, many of the most prominent of the symptoms of fever can only be explained by reference to the brain exclusively. Such is the case with regard to the disturbance observed in the different organs of sense, in that of the mental functions, and in the voluntary power—all of which are constantly

observed to be more or less disordered in every case of proper fever, and that always in proportion to the violence and danger of the disease; whereas other organs suffer occasionally only; and, when such is the case, it serves only to render the disease complicated in its form. The admission of the authors themselves goes far towards establishing the point in question; for they observe,[1] "that, either primarily or secondarily, we know the brain to be more or less affected in every case of fever; the affection, varying in intensity, being scarcely less manifest in the mild than in the severe, in the early than in the later periods of the disorder." This admission, if well founded, naturally leads to the suspicion that the brain is essentially, and not casually, concerned in fever; more especially when it is considered that no other organ than the brain is so constantly affected, and so decidedly deranged in its functions.

Thus, it appears that the difference between us is little more than verbal; for, by merely substituting the term *brain* for that of *nervous system*, as employed by the authors, the discrepancy in a great measure vanishes.

On the other hand, the supposition that the whole nervous system is equally involved with the brain, in the production of the phenomena of fever, is attended with many difficulties. It is a point by no means easy of proof, if conceded; for there are not the same means of discovering an affection of so many different parts, as the nervous system altogether is composed of, as where the brain singly is affected. The admission would, in fact, prove too much. A simultaneous morbid affection of the whole nervous system, comprising, under this denomination, the brain or encephalon—the spinal cord and the nerves, both cerebro-spinal and ganglionic, with their appendages—might be expected to give rise to a far more varied train of symptoms than is actually found to take place in proper fever. We might expect, for example, to find convulsions, tetanus, spasms in various parts, and paralysis of different kinds and degrees, as ordinary attendants of fever; whereas, in reality, such symptoms are of very rare occurrence; and, when they do occur, they are easily explained by reference to the brain alone, or, at most, by the supposition of an extension of the disease down the spinal cord, which sometimes seems to take place.

With respect to the second point of the theory of fever, the nature of that affection of the brain which is allowed "to be present in every case of fever, in the mild as well as in the severe, in the early as well as in the later periods of the disorder," the authors remark as follows:—"We are altogether unacquainted with the actual condition of the brain in the earlier stages of fever; and although more or less of excitement of the organ always succeeds to reaction, we have no evidence that that excitement amounts to inflammation in every instance. That inflammation of the organ, or of its membranes, does frequently occur, is proved alike," they further observe, "both by the symptoms during life, and by dissection after

[1] Elements, p. 82.

death; but we are in possession of no positive or infallible tests, by which to determine its existence or non-existence, in every case; neither can we ascertain, with precision, how much of the cerebral disturbance results from the actual condition of the encephalon itself, and how much of it depends upon mere sympathy with other suffering organs."

It cannot fail, I think, to be remarked here, in regard to the passages just quoted, that the authors are far from positively denying the existence of inflammation in the brain in fever in general; they only say, that we are unacquainted with the actual condition of the brain in the earlier stages of the disease; they allow that there is more or less of excitement present, but that we have no evidence that that excitement amounts to inflammation in every instance; yet they admit that inflammation of the organ, or its membranes, does frequently occur, as proved both by symptoms and by dissection. They observe, "that we are in possession of no positive or infallible tests, by which to determine its existence in every case." Now, there are but two modes of ascertaining the existence of inflammation in any case: one, by symptoms during life; the other, by dissection after death. The latter test (dissection) is, of course, not applicable to the milder forms of the disease, for these do not terminate fatally; and, therefore, it is only by symptoms that the case can be judged of. The severer and more dangerous forms of the disease are marked by well known symptoms, which are here placed in the strongest light; and in these cases, inflammation of the brain is allowed to be present. "The most decided marks of inflammation of the brain, or its membranes, in fever," it is observed, "are commonly met with, at an early period after reaction, in young adults of good constitution and full habit of body. They occasionally present themselves so early as the third or fourth day of fever, and consist of acute pain within the head, wildness and suffusion of the eyes, intolerance of light and noise, extreme restlessness, violent delirium, a frequent sharp pulse, a dry, parched, and generally hot skin, and a dry and brown (more rarely a moist) tongue. If these symptoms be not promptly checked, the patient falls into a state of exhaustion; he lies prostrate in bed, moaning, muttering, or uttering faint cries of distress; rolling about his body, picking the bed-clothes, and, when sufficient streng thremains, manifesting a remarkable degree of jactitation. At length the eyes roll about in their sockets, cold clammy sweats appear, the urine and stools pass off involuntarily, the pulse sinks, and death quickly follows."

Now, if this state of things, as occurring in the severer cases of fever, is so unhesitatingly admitted to be characteristic of inflammation of the brain, I do not see how the existence of inflammation can be questioned in the earlier and milder stages of the disease; for the symptoms are similar in nature in both cases, and differ only in degree. The mildest cases of fever, at first, are often seen to degenerate into the most severe, and that by gradual and almost imperceptible changes. There is no visible line of demarcation by which the early and mild is separated from the later and severer

stage of the disease, so as to enable us to mark precisely the actual accession of inflammation.

I am tempted to quote here another paragraph, because it bears still more strongly on the question. It alludes to fever in its ordinary state, such as is met with almost daily; whereas the statement before made, applies to what may be called an extreme case, and is therefore of less weight in argument. "It is probable," the authors remark, "that the aggravation of symptoms, which not unfrequently takes place about the eighth or tenth day of an ordinary mild fever, is owing, in many instances, to a corresponding, though less violent, inflammatory condition of the brain. The delirium, indeed, is less violent; but the great restlessness, the increase of languor and prostration, the dull expression of the eye, the feeble voice, the dry tongue, the frequent sharp pulse, and occasional starting of the tendons, sufficiently declare a state of brain which, if it be not actual inflammation, is one which is closely allied to it, both in its nature and its consequences. Neither is it unlikely that in fever, as in idiopathic inflammation of the encephalon, the symptoms are different accordingly as the membranes, or the brain itself, happen to be principally involved; the violent excitement appertaining chiefly to the former; the more depressed condition to the latter."

That the symptoms of the early stage, as here described, are the same in nature, and therefore indicate a similar origin, may be gathered from the following:—"Pain in the forehead or occiput, noise in the ears, confusion, giddiness, a sense of weight in the head, together with corresponding mental infirmity, and even delirium, are symptoms, some of which are almost uniformly present in the early stages of every fever, and, when moderate in degree, are such as neither bespeak organic change, nor portend danger."

It may be worth while to quote here the remarks of the authors on the subject of what they term simple inflammatory fever, identical with the synocha of Dr. Cullen. The consideration of this is placed at the head of the phlegmasiæ, with which it is said to be closely allied, and with which it is frequently combined. They endeavour to draw a distinction between this, the synocha, or, as they term it, simple inflammatory fever, and common continued fever, before described; from which it is said to differ chiefly "in the abruptness of its attack—in the early development of acute vascular excitement—in its shorter duration, and in the comparatively inconsiderable loss of muscular and vital power which attends or results from it." Nevertheless, it is allowed "that in some rare instances, it has appeared to pass into a form of fever not distinguishable from common continued fever." In fact, there is no essential distinction between them—the difference is in degree only. In the description given of this simple inflammatory fever, this synocha, as Dr. Cullen calls it, the characters of pyrexia, or symptomatic fever, are confounded with those of proper or idiopathic fever; and hence the difficulty of distinguishing between the two. "The flushed face, the suffusion of the eyes, the pain or

throbbing at the temples or within the head, the aching, uneasiness, and general soreness over the whole body, the great restlessness, watchfulness, and sometimes delirium, during the night," are all referable to the brain, as they manifestly consist in a disordered state of the functions of this organ; acccompanied with that increased arterial action in and about the head, which denotes the inflammatory state of the part. The other symptoms are merely those of pyrexia, or symptomatic fever.

In short, the real and essential difference between the mildest and the most malignant form of fever, appears to consist merely in the degree and extent to which the brain and its functions are suffering. In the milder form, and at the outset of the disease, (and that even in many cases where the most malignant symptoms appear afterwards), the brain suffers but little. The inflammation is slight in degree, and is therefore attended with but little disturbance of the sensorial functions. The febrile symptoms (pyrexia) may, indeed, run high, as in what is here called the simple inflammatory fever—the synocha of Dr. Cullen—yet with little danger to the patient, when properly managed. By active antiphlogistic treatment, the disease generally subsides altogether, (as it often does, indeed, spontaneously,) and that within a very limited period. Should this not be the case, the common febrile symptoms (the pyrexia, or general vascular excitement) decline in some measure, but the brain affection continues and gradually increases. The disease then takes the name of synochus. The brain suffers more and more, both in its structure and functions. The proper symptoms of fever continue to increase, until, in extreme cases, they acquire the highest degree of malignity, and thus the patient is destroyed; not, as has been supposed, from simple debility, but from the local organic mischief suffered by the brain.

It is no doubt true, that we are unacquainted with the actual condition of the brain in the earlier stages of fever; that is to say, we cannot have demonstrative proof on the subject, because the disease does not then destroy life, and dissection, consequently, is of no avail. We have, however, the evidence afforded by symptoms, and which is scarcely less conclusive. Pain, accompanied as it is in this case by heat and throbbing of arteries, and marked disorder of functions, are signs from which we infer the existence of inflammation in any part; and if such signs be further accompanied by a febrile state of system, the evidence is deemed conclusive; and such we have in the earlier as well as the later stages of fever. The aid of dissection can only be had in the more violent and fatal cases of the disease, in which the symptoms, as admitted by the authors, "sufficiently declare a state of brain, which, if it be not actual inflammation, is one that is closely allied to it, both in its nature and its consequences."

Dissection itself, however, is not at all times conclusive, in regard to the evidence it is capable of affording as to either the seat or nature of fever. In the first place, because fever patients do not always die from the fever itself, but from some supervening

disease, most frequently from inflammation in the abdomen; and in such cases the proper symptoms of fever may subside before death, and, of course, the disease would leave no traces behind it of the previous brain affection. And even where the patient has been actually destroyed by the inflammation of the brain, the post-mortem appearances are not always so strongly marked as to excite much attention on the part of those who are not very familiar with morbid anatomy. Visible alteration of structure, it must be remembered, is a remote and not an immediate effect of inflammation in the brain. The disease kills by the violence of action, and consequent interruption of functions, and not by any alteration in structure, which the eye is always capable of appreciating. The structure becomes changed slowly and gradually, in proportion to the duration, rather than the violence, of the disease. Hence all the most violent cases of inflammation of the brain, when they prove quickly fatal, whether in the form of acute hydrocephalus, or phrenitis, or fevers of unusual rapidity and violence, leave behind them in the brain the faintest traces of the preceding malady. It is from symptoms, therefore, and not from dissection, that the most satisfactory evidence is derived on such subjects.

Upon the whole, it may be observed, that the points which make the subject of the foregoing comments are of no small importance in a practical view. If the mild and early stage of fever be, as I believe, merely the commencement or first stage of that inflammation of the brain which the authors of the "Elements of Medicine" allow to be an almost invariable, if not an essential, accompaniment of the severer and more advanced stages of the disease, then it is of the greatest moment that the active measures they suggest, as applicable to this state of things, should be resorted to at the very outset; by which so dangerous a state may often be anticipated, as it were, and prevented; and such, I have ample reason to know, will generally be the result.

PATHOLOGICAL AND SURGICAL OBSERVATIONS

RELATING TO

INJURIES OF THE SPINAL CORD.

BY SIR BENJAMIN C. BRODIE, BT., F.R.S.,

SERGEANT-SURGEON TO THE KING, AND SURGEON TO ST. GEORGE'S HOSPITAL.[1]

SECTION I.

As the spinal cord is an appendage of the brain; as the functions of the one are intimately connected with those of the other; as they are constructed alike of medullary fibres and cineritious substance, enclosed by similar membranes, and protected, each of them, by a strong bony case; it seems reasonable to expect that they should suffer, nearly in the same manner, from the effects of mechanical injury. Pathological and surgical writers (with very few exceptions) seem to have taken nearly this view of the subject, and hence probably it is, that while their works contain abundant information respecting the consequences of wounds and contusions of the head, so little can be learned from them as to the not less important consequences of the same accidents, where they affect the column of the vertebræ.

It is with a view to contribute my share towards remedying this defect in surgical literature, that I venture to offer the following observations to the Royal Medical and Chirurgical Society. Those who have been much engaged in the practice of large civil and military hospitals will, I trust, discover in them no inaccurate representation of what they themselves have witnessed; while to the younger members of our profession, and to those whose opportunities are less extensive, it may be neither useless nor uninteresting to have placed before them a connected history of a class of accidents which they must occasionally meet with; which are always attended with danger, and which at the same time lead the mind of the scientific enquirer to a variety of curious physiological and pathological speculations.

It is my intention not to occupy the time of the society by giving a detailed account of individual cases, but rather to explain the

[1] Medico-Chirurgical Transactions, vol. xx. p. 118. London, 1837.

general results of my experience; it being understood that, except where I expressly refer to the observations of others, I make no statements for which I do not find an authority in the written notes of cases, which I have been enabled to collect in the course of the last few years.

Wounds, which penetrate through the external parts into the spinal cord, are almost invariably fatal at a very early period; the examples of recovery from them, which have been recorded by writers, being very few in number, and leading to no important rules of surgical practice. On this part of the subject I have nothing new to offer from my own experience. Those who will take the trouble of referring to M. Ollivier's treatise on diseases of the spinal cord, will find in it a notice of some interesting experiments illustrating the effects of these injuries in animals, as well as the histories of some cases, chiefly collected from other authors, in which they occurred in the human subject.

In considering the other and more common varieties of injury of the spinal cord, I shall, in the first instance, as the best foundation of our knowledge, endeavour to describe the effects of these accidents, as they are disclosed to us by dissection, where the patient has died at an early period after the infliction of the injury. Of course it is necessary to include in the description not only the effects produced on the cord itself, but also those on the parts which are in immediate connection with it. The whole of these may be classed under one or another of the following heads:

First. Fractures of the vertebræ without displacement of the fractured surfaces.

Secondly. Fractures with depression or displacement of bone, diminishing the diameter of the spinal canal, and occasioning pressure on the spinal cord.

Thirdly. Fractures complicated with dislocation.

Fourthly. Dislocations not complicated with fracture.

The existence of such dislocations has been doubted by some very distinguished surgeons, but Mr. Lawrence, in a paper on the subject, published in the thirteenth volume of the "Transactions of this Society," has not only recorded several cases in which they had taken place in the cervical vertebræ, but has also adduced satisfactory evidence of their existence even in the dorsal and lumbar portions of the spine. In a case which came under my own observation, the fourth and fifth cervical vertebræ had been completely separated from each other, so as to expose the *theca vertebralis* for the extent of half an inch. Similar cases of diastasis have been described by Sir Charles Bell and Mr. Lawrence.

Dislocations of the odontoid process of the second cervical vertebra in consequence of disease, are not very uncommon, so that probably there are few surgeons of much experience, who have not had the opportunity of witnessing cases of this kind.[1]

[1] See Rust, in Arthrokakologia: Lawrence, op. cit.; and my own Treatise on the Diseases of Joints.

Fifthly. Extravasations of blood on the surface of the membranes of the spinal cord. Such extravasations, however, rarely take place to any considerable extent, and bear no comparison to those which occur within the cavity of the cranium in consequence of a rupture of the substance of the brain, or a laceration of the middle meningeal artery.

Sixthly. A narrow clot of extravasated blood is sometimes discovered within the substance of the spinal cord. It is always of a very small size, but from its peculiar situation may be productive of the most dangerous symptoms.

Seventhly. Laceration of the spinal cord and its membranes. Of course it is more easy to conceive than to describe all the varieties of this kind of injury which are met with in practice. The spinal cord may be separated through its whole substance; or it may be torn in one part and not in another. M. Ollivier describes a case in which the attachments of the nerves on one side were destroyed, while those on the other side were entire.

Eighthly. The minute organisation of the spinal cord may suffer from a blow inflicted on the spine, even where there is neither fracture nor dislocation, and where the investing membranes do not appear to participate in any way in the effects of the injury. In such cases, if there be an opportunity of examining the spinal cord at a very early period after the accident has occurred, the central part of it is found to be softer than natural, its fibrous appearance being lost in that of a semifluid substance. If the patient survives for a longer period, the alteration of structure is perceptible in the whole diameter of the cord, and occupies from one to two inches, or even more, of its length; and at a still later period it has often proceeded so far as to terminate in its complete dissolution.

This disorganisation, softening, and final dissolution, of the spinal cord, is the most common consequence of injuries of the spine, and the dangerous symptoms which follow these accidents are, in the majority of cases, to be attributed to it. It bears no distant resemblance to the effects of a contusion of those soft parts which are more superficially situated, and it is easy to understand that it may be produced by a severe concussion operating on the delicate medullary fibres and cineritious substance, of which the spinal cord is composed.

In a paper which I communicated to this society in the year 1828, and which has been published in the fourteenth volume of the "Medico-Chirurgical Transactions," I have observed that in what are commonly called cases of concussion of the brain, we are not justified in the conclusion, that because no changes are to be detected after death, "there is, therefore, in reality, no organic injury." And I have added, "It is difficult to conceive in what other manner concussion of the brain can operate, so as to produce the effects which it is known to produce: and if we consider that the ultimate structure of the brain is on so minute a scale that our senses are incapable of detecting it, it is evident that there may be changes and alterations in it, which our senses are incapable of

detecting also. The speedy subsiding of the symptoms in some cases of concussion does not contradict this opinion. A deep incised wound in other parts may, under certain circumstances, be completely and firmly united in the space of twenty-four hours, and it is easy to suppose that the effects of a much slighter injury may be repaired in a much shorter space of time." These remarks are not less applicable to cases of concussion of the spinal cord than they are to those of concussion of the brain. We cannot doubt that the nature of the injury is the same in both of them. It is true that much worse consequences usually arise from concussion of the spinal cord than from concussion of the brain; that is, if the patient recovers, his recovery is more tedious: that if he dies, greater changes in the condition of the injured part are detected on dissection. But these differences in the effects of the injury are easily explained. The brain and its membranes completely occupy the cavity of the cranium, while the spinal cord and its membranes occupy only a part of the vertebral canal: and this being the case, it requires no great knowledge of mechanics to enable us to understand why the same degree of violence applied to the head, and spine, should occasion different degrees of mischief to the organs which they contain.

The effect of a violent concussion is at once to impair, and even to destroy, the functions of the spinal cord, sometimes even causing the patient's death in the course of a few hours; and the question here presents itself, "What is the nature of the injury thus inflicted on the spinal cord, so trifling in appearance, so great in reality, which is capable of producing such important and dangerous consequences?" It would be an interesting experiment, but it is one which I have hitherto neglected to institute, where a patient has died at an early period after an accident of this kind, to prepare the spinal cord by maceration in alcohol, and to endeavour, by tracing its fibres, to ascertain in what respects they are altered from their natural condition. The process of softening and dissolution of the cord which takes place afterwards, has, I know, been regarded by some pathologists as the consequence of inflammation, but a consideration of the following circumstances leads me to doubt the accuracy of this conclusion. 1st. A minute examination of the injured part of the spinal cord will often enable us to detect the commencement of the softening process at a very early period, before sufficient time had elapsed for inflammation to become established, and before any symptoms of inflammation had shown themselves. 2d. The softened part of the cord, in the first instance, exhibits no appearance of increased vascularity. 3d. Even where the softening process is so far advanced as to occasion complete disorganisation of the spinal cord, the investing membranes, for the most part, exhibit their natural appearance, there being neither increased vascularity, nor the slightest effusion of lymph, or serum, or pus, on their surface. 4th. The symptoms, which mark the progress of these changes, are (as I shall show hereafter) merely a continuation of those which the concussion of the spinal

cord has occasioned in the first instance, and which, of course must have been wholly unconnected with inflammation.

It is true that the disorganisation of the spinal cord never proceeds to any considerable extent without an enlargement of the small vessels being perceptible, such as may be supposed to indicate the existence of inflammation: but this is no more than what happens in the progress of any other disease. As far as I know there is no exception to this general rule. A scirrhous or fatty tumour is not the result of inflammation, but inflammation, nevertheless, takes place sooner or later, as the disease makes progress. Nothing can be more remote from inflammation than neuralgia is in its origin; yet a slight degree of inflammation frequently takes place where neuralgia has been constant and of long continuance. In these, and in a number of other cases which might be enumerated, the inflammation is not the cause, but the effect or concomitant, of the disease, whatever it may be.

In further confirmation of the opinions which are now expressed, it may be observed that there is a manifest resemblance between the softening of the spinal cord, which follows mechanical injury, and that softening of the brain and spinal cord which takes place from internal causes, and which was first particularly described by M. Rostan, under the name of *rammollissement du cerveau*. M. Andral properly rejects the notion that this change is the mere result of inflammation, and every practical pathologist must have had ample opportunities of determining the justice of the argument which he uses, that "We often meet with cases (of *rammollissement*) where there is no appearance of sanguineous congestion, nor of purulent effusion, nor of any morbid secretion; a simple diminution of consistence being all that is to be seen."[1]

Although the softening of the spinal cord is not the consequence of inflammation, we must not overlook the circumstance, that inflammation of the investing membranes sometimes exists in combination with it, or that such inflammation may take place where the cord is little or not at all affected in this manner. Inflammation, however, is to be regarded as one of the secondary effects of the injury, and the consideration of it belongs more properly to another part of the present investigation.

SECTION II.

The peculiar symptoms which arise as an immediate consequence of injury of the spine, may be referred, 1st, to concussion of the spinal cord; 2d, to laceration or division of its substance; 3d, to

[1] A treatise on Pathological Anatomy by G. Andral, translated by Dr. Townsend and Dr. West, vol. ii. p. 748. 1831.

pressure made on it either by displacement of bone or extravasated blood. Afterwards, inflammation of the membrane of the cord may take place, and other organs may be secondarily affected, giving rise to another order of symptoms, which did not exist in the first instance.

Taking a general view of the symptoms, we may observe that they vary; 1st, according to the part of the spinal cord on which the injury has been inflicted; 2d, according to the kind and degree of injury which the cord has sustained; 3d, accordingly, as from accidental circumstances, that is from circumstances of which we cannot take cognisance, the life of the patient is prolonged for a shorter or longer period, or ultimately preserved.

Such is the diversity of the symptoms which we meet with in practice, that it would be vain to attempt to give a single connected history of them, which would be applicable to all, or even the majority of cases: and, on the other hand, a needless repetition in many respects would be the result, if we were to endeavour to distribute the cases under different heads, accordingly as the symptoms arise from different degrees and kinds of injury, or from injuries occurring in different situations. The best consideration which I have been able to give the subject, leads me to believe that the following method of analysis will (on this occasion) be less liable to objection than any other. I propose, in the first instance, to give an account of the several symptoms, which are the consequence of injuries of the spinal cord, in succession, endeavouring to point out (as far as it is in my power to do so) under what circumstances each of them presents itself; and afterwards to offer some general observations, with a view to connect the whole together, and complete the history, which might otherwise be deficient. I hope thus to be enabled to avoid the omission of any facts which are really important, without sacrificing that brevity and conciseness, which it is so important to preserve in all scientific investigations.

SYMPTOMS OF INJURIES OF THE SPINAL CORD.

Paralysis of the Voluntary Muscles.—The most obvious effect of a severe injury of the spinal cord, is a paralytic state of the voluntary muscles below the seat of the injury.

If the spinal cord be divided through its whole substance, or extensively lacerated, or subjected to any considerable degree of pressure, the paralysis is immediate and complete. If the injury be partial, certain muscles may be paralysed, while others retain their power of voluntary motion.

Concussion of the spinal cord often produces complete paralysis also; but more frequently the paralysis arising from it is partial. One limb may be paralytic and another not so; or in the same limb certain muscles may be thus affected, while others are still obedient to the will. In some cases the patient has the power of using his limbs while he is in the horizontal posture, yet he is unable to stand

erect. Or the degree of paralysis may vary at different periods. Thus it may be complete at first; then, after three or four days, the power of motion may be in some degree restored; then it may be lost again. Sometimes, although the paralysis is complete, or nearly complete, in the first instance, so speedy a recovery takes place, that the patient is able to walk in the course of three or four weeks, or even sooner; or the converse of this may happen, so that, although there is no more than a weakness of the muscles immediately after the accident, complete paralysis may take place, sometimes gradually, at other times almost suddenly, after the lapse of several weeks.

Paralysis of the lower limbs is more common than that of the upper. In some cases, in which the injury has affected the spinal cord in the lower part of the neck, the lower limbs are rendered paralytic, while there is either no paralysis, or a less degree of paralysis in the upper limbs. The reason of this is sufficiently obvious to any one who considers what are the origins of the nerves which form the axillary plexus, some of them being probably above the part which is injured: but the circumstance is still remarkable in this respect; that it is contrary to what happens when the functions of the spinal cord are interrupted in consequence of caries of the cervical vertebræ. In these last mentioned cases the paralysis is often complete in the upper limbs for many weeks, or even for months, before it extends to the lower. I have met with only a single case in which, after an injury of the cervical portion of the spine, there was almost complete paralysis of the muscles of the upper extremities, and none whatever of those of the lower.

A case is recorded by Mr. Stafford, in which there was paralysis of both the upper and the lower limbs, consequent on an injury of the loins, with fracture and displacement of the lumbar vertebræ; and another case fell under my own observation, in which paralysis of the upper limbs followed a contusion of the middle dorsal vertebræ. I conclude that such cases are no more than apparent exceptions to the general rule, of the paralysis being confined to the parts which are below the injured portion of the spinal cord. It is easy to suppose that the bones may be fractured or displaced in the loins or back, or that blood may be extravasated within the theca vertebralis in one or the other of these situations, while another part of the spinal cord, as high as the origin of the nerves which form the axillary plexus, suffers from the effects of concussion.

Paralysis, after an injury of the spine, is always a dangerous symptom; but many persons thus affected recover nevertheless. For example: a gentleman was thrown from his horse, and received a severe blow on his back. Five weeks afterwards he became paralytic in his lower limbs; but at the expiration of fifteen or sixteen weeks more, he began to recover the use of them, and he was able to walk, with the assistance of a stick, when I was consulted about a year afterwards. I have known many cases very nearly resembling this in all essential circumstances. A

patient in St. George's Hospital, whose lower limbs were paralytic after a severe blow on his spine, regained the use of them in the course of three or four weeks. I shall have occasion to revert to this subject hereafter: in the mean time I may observe, that it is easy to understand that, where paralysis is produced by the pressure of extravasated blood, it may be relieved by the absorption of the coagulum; or that the injury inflicted, by concussion on the structure of the spinal cord, may be gradually repaired.

A case is related, by Morgagni, of a young man who was wounded by a dagger in the neck, and immediately became paralytic in all the parts below the injury. Nevertheless, in less than six weeks he began to re-acquire the use of his muscles, and he was able to walk slowly and cautiously at the end of four months. The records of surgery contain histories which are still more remarkable, in which the spinal cord is said to have been completely divided, without the production of paralysis either at the time or afterwards. Such histories, however, are rare,[1] and until they are confirmed by further observation, it must remain as a question, whether it be more probable that there should have been so remarkable a deviation from the ordinary course of events, or that the state of the spinal cord, in those cases, should have been carelessly or insufficiently examined. Undoubtedly our judgment respecting them is not assisted by the analogy of those other cases, which have been described by Magendie, Velpeau, and others, in which the circumstance of a portion of the spinal cord having been reduced to an apparently semifluid state without consequent paralysis, may be explained by supposing that there were, nevertheless, sufficient remains of the medullary fibres to answer the purpose of transmitting the nervous influence.

Muscular Spasms.—A gentleman, in October, 1827, was thrown from his horse, which afterwards rolled over him. His head was not struck, and the principal injury seemed to have fallen on the lower part of the spine. Soon after the fall, and while he was yet lying on the ground, his thighs were raised spasmodically towards the trunk, and this was followed by an involuntary tremulous motion of the lower limbs. Afterwards the lower limbs became paralytic, and continued in that state for a period of two months. At the end of that time he began to recover, and when I was consulted, about a year after the accident, he was able to walk without even the assistance of a stick.

In the observations on injuries of the brain which I formerly communicated to this society,[2] I mentioned some circumstances which seem to show that the convulsions which occasionally occur at an early period after a severe blow on the head, are the consequence of a slight extravasation of blood, which is sufficient to operate as a cause of irritation, without actually destroying the

[1] I refer especially to two cases, quoted by Velpeau, from the Mémoires de l'Academie des Sciences and Desault's Journal.

[2] Medico-Chirurgical Transactions, vol. xiv., p. 370.

functions of the brain. Whether the convulsions which took place in this instance had a similar origin, or arose from that disorganisation of the spinal cord which is the most usual cause of paralysis after concussion, must be left to future observations to determine. The same symptoms sometimes occur at a later period. For example: a man, forty-five years of age, in January, 1825, fell from a scaffold, and received a blow on his back. All the parts below the epigastrium became immediately paralysed. At the end of nine days it was observed, for the first time, that a slight involuntary action of the muscles of the thighs was induced when pressure was made accidentally on these parts. Afterwards severe cramps and painful convulsions took place, whenever pressure was made on any part of the body, or even by lifting up the bed-clothes. At last they were almost constant, so as continually to awaken him from his sleep. When he died, nine weeks after the accident, it was ascertained that there had been a fracture of the fourth dorsal vertebra, with such a degree of displacement as to produce a slight degree of pressure on the spinal cord. There was an abscess, containing from four to six ounces of pus, communicating with the fracture, and extending into the posterior mediastinum. The membranes of the spinal cord, and the spinal cord itself, presented a natural appearance externally, but on the latter being divided longitudinally, the central part of it was found to be in a softened state, so that on being macerated for a short time in water it almost completely disappeared.

A boy was admitted into the hospital in September, 1827, with a fracture and considerable displacement of the third and fourth lumbar vertebræ, so as to cause a manifest alteration in the figure of the spine. He was paralytic in his lower limbs. An attempt was made to restore the displaced vertebræ to their natural position, and was attended with some, but not complete, success. At the end of a month he became affected with slight involuntary motions of the lower limbs, and at the same time he began to recover the power of moving them voluntarily. Early in the following January he quitted the hospital, and I have had no opportunity of seeing him or hearing of him since.

In each of the two last mentioned cases there was some degree of pressure on the spinal cord; and I am the more inclined to believe that this was the cause of the spasmodic affection of the muscles, as I have not met with any case in which it was proved by dissection, that this symptom existed in combination with disorganisation of the cord, and independently of pressure on it.

Affections of the Nerves of Sensation.—If the spinal cord be lacerated, or subjected to any considerable degree of pressure, the sensibility of the parts below the seat of the injury is totally destroyed. If the injury be in the situation of the sixth or seventh cervical vertebra, the destruction of sensibility is frequently partial in the upper extremities, while it is complete in the trunk and lower extremities; but if it correspond with the two vertebræ immediately above these, the patient, during the short remaining period of his

life, presents the extraordinary phenomenon of a living head, with its sensibility and muscular powers unimpaired, attached to a trunk and extremities of the existence of which he is totally unconscious.

In cases of concussion of the spinal cord, there are the same varieties with respect to the destruction of sensibility, as there are with respect to that of the power of voluntary motion. Sometimes it is every where complete; at other times it is complete in one part and not in another, or the sensations may be dull in every part of the body, although not totally destroyed. Sometimes the skin appears to be insensible, the patient being, nevertheless, conscious of pressure made on the more deeply seated parts.

Not unfrequently, unusual sensations are referred to parts, the nerves of which are actually incapable of conveying to the sensorium the impressions made by mechanical pressure, or the application of heat. Sometimes the patient complains that he feels as if he had been every where severely bruised; or he has a sense of burning, or of tightness and constriction. In many instances, the destruction of sensibility is incomplete at first, but becomes complete afterwards, as the process of softening makes progress in the injured portion of the spinal cord. Where recovery takes place, the restoration of sensibility usually precedes that of the power of voluntary motion; so that the patient may be quite sensible of external impressions, while he is still incapable of employing his muscles for any useful purpose. The last observations apply equally to all cases, whether the spinal cord has suffered from concussion, or from the pressure of displaced vertebræ.

Affection of the Respiration.—It is well known that if the spinal cord be divided or lacerated above the origin of the phrenic nerves, that is, above the situation of the third cervical vertebra, immediate death is the consequence. Under these circumstances the nervous influence is no longer transmitted either to the diaphragm or to the other muscles of respiration, and the animal perishes in the same manner as from strangulation. If respiration be performed artificially, by inflating the lungs, the action of the heart may be maintained, so as to cause life to be prolonged for several hours.

Pressure made on the superior portion of the spinal cord, in consequence of dislocation or fracture, is attended with a similar result. A case of sudden death from dislocation of the second vertebra is recorded by Petit, and other similar cases are described by Sir Charles Bell and Mr. Stafford. The latter author mentions two cases of death taking place immediately, from fracture of the second and third cervical vertebræ. I attended a young gentleman who laboured under symptoms of caries of the superior cervical vertebræ, and who, after having eaten a hearty dinner, suddenly expired while altering his position in bed. The body was not examined, but there was good reason to believe that the spinal cord was compressed in consequence of a dislocation of the odontoid process of the second vertebra.

Dislocations of the first and second vertebræ do not, however,

prove fatal in this manner in every instance. A most remarkable example of this fact has lately been communicated to the society by Mr. Phillips. In this case, the dislocation was the result of accidental violence. A child, four or five years of age, was admitted into St. George's Hospital, labouring under well marked symptoms of caries of the cervical vertebræ. The function of respiration was unimpaired. Soon after her admission other symptoms supervened, similar to those which arise from hydrocephalus, and she died as if from pressure on the brain. On examining the body, the ventricles of the brain were found to be much distended with fluid. The transverse ligament of the second cervical vertebra had given way, and the odontoid process formed a considerable projection into the spinal canal. The dura mater, however, was entire, and prevented the dislocation being so complete as it would have been otherwise. The pressure on the spinal cord was not sufficient to destroy its functions, although it might well be supposed to have operated as a cause of irritation, so as to produce the effusion into the ventricles of the brain. Another patient, a lad about sixteen years of age, who had been admitted into St. George's Hospital, labouring under a caries of the cervical vertebræ, died under similar circumstances.

When the spinal cord is seriously injured in the lower part of the neck, (that is below the origin of the phrenic nerves,) or in the upper part of the back, there is nothing to interfere with the due action of the diaphragm, while the intercostal muscles, as well as the muscles of expiration, are rendered paralytic. The patient therefore breathes by the diaphragm only. The ribs are motionless, and the air is expelled from the lungs, not by the contraction of the abdominal muscles, but simply by the elasticity of the abdominal parietes, and the pressure of the abdominal viscera operating on the lower surface of the diaphragm, where that muscle is relaxed. It may be presumed that under such circumstaces expiration is never so complete as where it is the result of muscular action. At all events, we may observe that the patient is incapable of expectorating mucus if it be collected in the trachea: that, if he coughs, the cough is peculiar, being effected by a forcible inspiration, followed by a sudden relaxation of the diaphragm; and that if he be placed in the sitting posture, so that the pressure of the abdominal viscera is removed from the diaphragm, he breathes with much greater difficulty that when he is lying down. All these are to be regarded as very formidable symptoms. Such imperfect respiration seems to be insufficient for the maintenance of life: the patient seldom survives so long as the sixth day, and he more frequently dies in less than forty-eight hours after the occurrence of the accident. Recoveries under these circumstances are very rare.

Injuries of the spinal cord in the lower part of the neck are not, however, necessarily followed by these results. The pressure on the spinal cord may be so small, or the disorganisation of it from concussion may be so trifling, that the muscles of respiration are not, at any period, affected by it; or they may not be so in the first

instance, and yet become affected afterwards. In one case which fell under my observation, and in which there was a fracture of the seventh cervical vertebra, followed by a softening and dissolution of the spinal cord, the difficulty of respiration did not take place until the twelfth day; but death occurred in less than three days afterwards.

In proportion as the injury affects the spinal cord lower down in the back, so the respiratory function is in a less degree impaired. In a case of fracture of the sixth dorsal vertebra, I observed (what indeed we might well expect to happen) that although inspiration was well enough performed, the abdominal muscles did not act in expiration. Wherever the injury is situated, a disposition to cough, with a copious expectoration, is likely to occur some time after the accident. In one case, in which there was a fracture of the eleventh dorsal vertebra, with softening of the spinal cord in the same situation, these symptoms began as early as the third day; and such was the disposition to cough, that it was induced by any slight change of position. Nevertheless, this patient survived until the end of the fifth week.

Priapism.—This is a very common symptom of injury of the spinal cord, and it is remarkable that, although under ordinary circumstances the erection of the penis is the result of an impression communicated from the sensorium, I have never known it to occur, in these cases, except in combination with paralysis. Priapism may take place whether the patient suffers from the effects of simple concussion of the spinal cord, or from those of pressure. It seems to be connected with injuries of the upper, rather than with those of the lower portion of the cord: at least, I am not aware that I have met with it where the seat of injury has been below the sixth dorsal vertebra. It is for the most part an early symptom, showing itself in the course of the second or third day, and it seldom continues after the first fortnight. It occurs even where the sensibility of the parts is totally destroyed, and may be induced by the mechanical irritation caused by the introduction of the catheter, where the patient is entirely unconscious of the operation. This circumstance was pointed out to me many years ago by Professor Macartney of Trinity College, Dublin; and I have had many opportunities of verifying the correctness of the observation.

Affections of the Urinary Organs.—Paralysis of the lower half of the body is, as might be expected, attended with an inability to void the urine: so that the patient requires the aid of the catheter. In the great majority of cases he is not sensible of any inconvenience; and indeed is totally unconscious of the bladder being distended: but there are exceptions to this rule, and occasionally he suffers as he would from an ordinary retention of urine, but in a less degree. The incapability of voiding the urine is usually an early symptom, and in fatal cases it continues to the last. When a complete or partial recovery takes place, the power of emptying the bladder is restored sooner than that of using the muscles of the lower limbs. In cases in which the catheter is not employed, the

urine flows involuntarily, as in most other cases of over-distended bladder. At other times there is a constant dribbling of urine, although the bladder is in a contracted state, so that on the introduction of the catheter no urine flows.

None of the consequences of injuries of the spinal cord are more remarkable than the changes which are produced in the secretions of the kidneys and bladder. My attention was first called to this subject as long ago as the year 1807; and my subsequent experience has enabled me to make the following observations on the subject.

The first effect of a severe injury of the spinal cord is not unfrequently to occasion a marked diminution in the quantity of urine secreted. This is most observable where the injury is in the lower part of the neck, and where, in consequence, the function of respiration is very much impaired. Thus, in a patient in St. George's Hospital, in whom there was a forcible separation of the fifth and sixth cervical vertebræ, complicated with fracture and depression of bone, and laceration of the spinal cord, four ounces of urine were drawn off by the catheter at the end of twenty-four hours after the occurrence of the accident, and when he died, at the end of twenty-six hours more, the same quantity was found in the bladder, none having been voided in the intermediate time. The same thing, however, may occur when the injury is in the lower part of the spine. For example: a gentleman received a blow on the loins, which occasioned, in the first instance, a partial paralysis of the muscles of the lower extremities. He never experienced any difficulty in voiding the urine, but the secretion was almost entirely, if not entirely, suspended during the first twenty-four hours. I did not see this patient at the time, but I cannot doubt the accuracy of the report which he made when I was consulted some time afterwards.

In some cases, the urine which is first secreted after the occurrence of the accident, although of an acid quality, and free from mucus, has a peculiarly offensive and disgusting odour. In other cases the urine is highly acid, having an opaque yellow appearance, and it deposits a yellow amorphous sediment. In one case this colouring matter was in such abundance that it was found after death to have imparted a yellow tint to the mucous membrane of the bladder, which at the same time bore no marks of inflammation, even exhibiting less appearance of vascularity than under ordinary circumstances.

But the most common change produced in the urine by an injury of the spinal cord is the following. It is voided of an ammoniacal odour, and turbid; when allowed to cool and remain at rest, it deposits a large quantity of adhesive mucus, and when tested with reddened litmus or turmeric paper, it is found to be highly alkaline. After some time a quantity of white matter (phosphate of lime) may be detected in the mucus, and it is tinged with blood. At a still later period a considerable quantity of coagulum of blood is blended with the mucus and urine. These appearances very

commonly show themselves as early as the second or third day after the occurrence of the accident; sometimes not before the end of a week, or even eight or nine days. I have not observed that injury of one part of the spine is more liable to produce them than injury of another. There is a great variety as to the period of their duration. In fatal cases they sometimes continue to the last, even though the patient should survive for several weeks, or even months; at other times they continue for two or three weeks, then subside, and the urine remains transparent, and of an acid quality afterwards. In other cases the quality of the urine varies almost from day to day, without any manifest reason for the change. It may be alkaline, depositing adhesive mucus; then clear and acid; then alkaline again; and these alterations may take place several times in the progress of the same case.

It is well known to pathologists that such adhesive mucus as is here described is never a constituent part of the urine as it is secreted by the kidneys, but that it is furnished by the mucous membrane of the bladder and ureters, especially of the former, when in a state of inflammation. The effect, therefore, of an injury of the spine is, in many instances, to occasion inflammation of the mucous membrane lining the urinary organs; and the consequences of such inflammation, where the urine has continued alkaline and loaded with mucus up to the time of the patient's death, are very manifest on dissection. The mucous membranes of the bladder, ureters, pelves, and infundibula of the kidneys is highly vascular; and in some cases the bladder is lined with phosphate of lime, which the mucus has deposited on its surface. Occasionally, spots of extravasated blood are found in the glandular substance of the kidneys, and loose masses of coagulum in the pelves of the kidneys and in the bladder.

An interesting question here presents itself, whether the inflammation of the mucous membrane of the bladder be a primary or a secondary disease? Whether the injury of the spinal cord operates directly on the mucous membrane, or whether its first effect is to alter the quality of the urine, as it is formed by the kidneys, the mucous membrane becoming affected afterwards, in consequence of the irritation excited in it by the contact of an unhealthy and stimulating secretion? These points remain to be determined by future observations.

Affections of the Digestive Organs.—In the first instance, whatever be the seat of the injury in the spinal cord, the bowels are torpid, so that they cannot be made to act except under the influence of the most powerful purgatives. Then the abdomen becomes tympanitic; and, in fatal cases, however protracted, these symptoms generally continue, in a greater or less degree, to the last.

The same state of the nervous system which produces costiveness, occasions incontinence of the fæces which have already reached the rectum, so that the evacuations take place involuntarily, without the consciousness of the patient. In many cases in which

the injury is in the cervical portion of the spinal cord, and death takes place in the course of two or three days, there is a diposition to vomit. In one case which fell under my observation, there was incessant vomiting of a large quantity of dark coloured fluid: in another case, in which the patient died on the fifth day, during the two days which preceded his dissolution there was a perpetual gulping, and ejection of a similar diseased secretion.

In more protracted cases the alvine evacuations are of a black colour; semifluid, somewhat resembling tar or treacle in their appearance, and of a peculiar and offensive odour.

It has not been ascertained, as far as I know, what is the nature of the substance on which this dark colour depends. It certainly is not mere extravasated blood. Probably it is furnished by the secreting vessels of the stomach and intestines, and corresponds to the dark coloured matter which is sometimes vomited at the termination of typhus fever, or to the black sordes which, under the same circumstances, are attached to the gums and teeth. It is only within the last two or three years that my attention has been directed to the subject, and further observations are required for its elucidation.

Alteration of the Vital Temperature.—M. Chossat has published an account of some experiments on animals, in which he found that the division of the superior portion of the spinal cord produced a remarkable evolution of animal heat, so that it was raised much above the natural standard. I have made experiments similar to those of M. Chossat, and have met with similar results. I have also seen several cases in which an accidental injury of the spinal cord has produced the same effect. The most remarkable of them was that of a man who was admitted into St. George's Hospital, in whom there was a forcible separation of the fifth and sixth cervical vertebræ, attended with an effusion of blood within the theca vertebralis, and laceration of the lower part of the cervical portion of the spinal cord. Respiration was performed by the diaphragm only, and, of course, in a very imperfect manner. The patient died at the end of twenty-two hours; and, for some time previously to his death, he breathed at very long intervals, the pulse being weak and the countenance livid. At last there were not more than five or six inspirations in a minute. Nevertheless, when the ball of a thermometer was placed between the scrotum and the thigh, the quicksilver rose to 111° of Fahrenheit's scale. Immediately after death the temperature was examined in the same manner, and found to be still the same.

In the year 1812 I published an account of some experiments made on rabbits, in which it was found that when these animals were stupified by the action of the Woorara poison, although the same quantity of oxygen gas was consumed in respiration as under ordinary circumstances, little or no animal heat was generated. In the case to which I have just referred, the effect was exactly the converse of that which was produced in these experiments; the respiration being so imperfect that the consumption of oxygen must

have been very much diminished, yet the production of animal heat was much greater than under ordinary circumstances.

Gangrene.—One result of an injury of the spinal cord is a diminution of the vital powers of the external parts, so that gangrene takes place, and sloughs are formed, on even the slightest pressure.

This disposition to gangrene is evidently a direct consequence of the injury of the cord, since it occurs equally whether the action of the heart be strong or feeble, and is limited to those parts which are below the seat of the injury.

In many cases in which the injury has affected the cervical portion of the spinal cord, sloughs begin to be formed, not only on the sacrum and nates, but even on the ankles, as early as the second day. In the last-mentioned parts we have the opportunity of watching the gradual formation of the sloughs. There is first a purple appearance of the skin ; then a vesication containing a dark coloured fluid ; and this is for the most part immediately followed by the loss of vitality in the subjacent textures. When the injury has affected the dorsal portion of the spinal cord, the sloughs are generally, but not always, confined to those parts on which the pressure is greatest ; that is, to the integuments of the nates and sacrum ; and the formation of them begins at a later period. It is remarkable that in these last-mentioned cases the separation of the sloughs begins at an unusually early period, fresh sloughs being formed immediately afterwards.

Affection of the Sensorium.—I refer in this place merely to the effect produced on the sensorium immediately after the accident has occurred.

I have seldom observed the sensorium to be materially affected, except where the injury was in the cervical portion of the spinal cord, and here the results are very different in different cases.

Thus, in one patient, in whom there was a fracture of the fifth and sixth cervical vertebræ, with displacement of bone and laceration of the spinal cord, the functions of the sensorium were in no degree disturbed, the patient being perfectly conscious and talking rationally. Another patient, in whom the same part of the cord was bruised and lacerated, became comatose soon after the accident. On some blood being taken from the arm, the coma subsided ; but at the end of twelve hours he became again comatose, and continued so until he died, ten hours afterwards. A third patient, in whom there was a fracture of the fourth and fifth cervical vertebræ, with softening of the spinal cord, was at first perfectly conscious and sensible. In less than twenty-four hours he fell into a state approaching to that of complete stupor ; then became delirious, and continued so until he died, thirty-six hours after the accident. A fourth patient, in whom there was a small extravasation of blood in the centre of the spinal cord, opposite the fifth and sixth cervical vertebræ, died in less than forty-eight hours, having been sensible and conscious nearly to the last, but the pupils of his eyes being contracted.

SYMPTOMS NOT INCLUDED UNDER THE FOREGOING HEADS.

The first effect which a severe injury of the spinal cord (whatever may be the exact nature and seat of the injury) produces on the circulation, is to lessen the force of the heart's action, and to cause a state of general depression and collapse; the pulse being very feeble, contracted, and sometimes scarcely perceptible. Occasionally a rigor takes place soon after the accident has occurred.

When the injury is in the lower part of the neck, the patient not unfrequently dies before complete re-action is established, the pulse remaining feeble to the last; or it may beat distinctly, but not oftener than fifty or sixty times in a minute. In the majority of cases, however, after the first twenty-four hours, the pulse rises to ninety-six or a hundred in a minute; but still it is feeble and contracted, indicating a state of great general debility rather than the existence of an active inflammatory disease. The appearance of the tongue corresponds to the character of the pulse; and it is not unusual at the end of twenty-four hours to find it dry and parched, covered with a brown fur, which is soon converted into a black crust, resembling what we observe in the last stage of a continued fever.

When the injury is in the lower part of the neck, but not of such a nature as to occasion death within the first three or four days, or where it has affected the dorsal or lumbar portion of the spinal cord, and the patient does not die at an early period, or ultimately recovers, the pulse usually remains for a considerable time more frequent than natural, varying from ninety to one hundred and twenty beats in a minute, but feeble and contracted, the tongue at the same time becoming more clean and moist than it was in the first instance. If blood be drawn under these circumstances, the coagulum is of a large size and loose texture; often never exhibiting any inflammatory appearances whatever; sometimes having a slight buffy coat on its surface in the first instance, but none afterwards. As far as I have seen, these observations apply to all those cases in which the effect of the injury is to induce that softening of the medullary substance of the spinal cord, which may or may not terminate in its complete dissolution, without exciting inflammation of the membranes in which it is enveloped; and these facts go far towards confirming the opinion which I have already ventured to express, that this change in the organisation of the cord is to be regarded as the result of a peculiar process, which may exist independently of inflammatory action.

Inflammation of the membranes of the spinal cord is undoubtedly a much more rare consequence of injuries of the spine, than inflammation of the membranes of the brain is of injuries of the head. In the cases of this kind which have fallen under my observation, the process of softening and dissolution of the spinal cord had gone on simultaneously with the inflammation of the membranes, and there were, during life, all those symptoms by which

the existence of the former of these affections is indicated in other instances. But superadded to these, there were profuse perspirations, and severe and repeated rigors, marking the occurrence of suppuration: there were also spasmodic twitches of the voluntary muscles, but not until it might reasonably be supposed that pus was collected in sufficient quantity to make pressure on the spinal cord. I have already mentioned cases in which pressure arising from other causes produced a similar effect.

Sir Charles Bell has described two cases, in which inflammation and suppuration of the membranes of the spinal cord followed injuries of the spine, unconnected with softening of the cord itself, and producing symptoms a good deal similar to those which are described as taking place where these membranes are inflamed from other accidental causes. In the first of these was a fracture of the eleventh dorsal vertebra. The patient was affected with delirium, attended with a rapid pulse. The most remarkable circumstance was, that there was no loss either of sensation or of the power of voluntary motion. Death, preceded by typhoid symptoms, took place on the fifth day.

In the other case recorded by Sir Charles Bell, the injury was in the lower cervical and first dorsal vertebræ, and the whole of the spinal cord, from this part to the lower part of the loins, was found after death to be bathed in pus. There were no severe or urgent symptoms for the first eight days; then the patient was seized with violent convulsions, followed by fever and delirium. There were no paralytic symptoms until the tenth day, when there was a difficulty in raising one arm, and this was followed by complete paralysis of the lower limbs two days afterwards. Death, preceded by typhoid symptoms, took place nearly three weeks after the occurrence of the accident.

These cases are of great interest, and among those of which I have preserved notes I can find none precisely similar to them.

I have met with one case in which inflammation of the membranes of the spinal cord took place under peculiar circumstances, being apparently not a primary but a secondary effect of the injury. A man was admitted into the hospital, in whom there was a fracture of the sixth dorsal vertebra; the consequence of a fall from a high scaffold. He died at the end of six weeks, with softening of the spinal cord, in the situation of the fracture, for the extent of two inches, and having laboured under the usual symptoms. On the tenth day after he met with the accident, there were vesications containing a dark coloured serum on one foot, and a slough had begun to form on the integuments over the *os sacrum*. The sloughing process rapidly extended, and about a week before he died, a large slough came away, including the greater part of one *glutæus maximus* muscle.

On examining the body after death, besides the softening of the spinal cord, already mentioned, in the middle of the back, the following appearances were observed. The sloughing process had extended so as to destroy the soft parts covering the *os sacrum* and

the parts in its vicinity, including the ligaments at the posterior part of the pelvis and those of the lumbar vertebræ. The spinal canal was, in consequence, exposed, and a considerable quantity of pus had been deposited between the vertebræ and the *dura mater*. On the *dura mater* being divided, a layer of coagulated lymph of a yellow colour was found adhering to the inner surface of that membrane, and to the external surface of the arachnoid. The lymph was most abundant in the situation of the *cauda equina*, but traces of it were perceptible as high as the lowest dorsal vertebra. From this part to the middle of the back, in which the spinal cord was in a half-dissolved state, the spinal cord and its membranes bore no marks of disease. Unless it were the existence of profuse perspirations, there were no peculiar symptoms which could reasonably be attributed to the extension of the sloughing, and the effects produced by it on the deep-seated parts within the spinal canal.

In reviewing the various consequences of injuries of the spinal cord, we find nothing more remarkable than the following circumstance: that whether the cord be lacerated or compressed, or has undergone that kind of disorganisation which is induced by a severe concussion, there is no material difference in the symptoms which arise, or in the results to which they lead.

There is another circumstance not much less worthy of notice than those which I have just mentioned. The great majority of the symptoms are the same, whatever part of the spinal cord has suffered from the injury. This observation applies to the state of collapse, which immediately follows the accident; paralysis of the voluntary muscles, loss of sensibility, priapism, the disposition to gangrene, the altered secretions of the kidneys, inflammation of the mucous membrane of the bladder, and consequent deposition of adhesive mucus by the urine, and the derangement of the functions of the digestive organs, as indicated by tympanitis, and the discharge of black and offensive evacuations from the bowels.

There is only one order of symptoms with respect to which a great difference exists accordingly as the seat of the injury is in one or another part of the spinal cord. If the functions of the cord be interrupted above the origin of the phrenic nerves, respiration is immediately suspended, and instantaneous death ensues. If the same thing happens in the lower part of the neck, or in the upper part of the back, respiration is performed by the unassisted action of the diaphragm: if in the middle or lower part of the back, the muscles of inspiration are unaffected, but those of expiration are paralysed. It is only in those cases in which the injury is trifling, or confined to the lowest portion of the spinal cord, that the respiration is altogether unaffected. These facts have been already stated, but they deserve our attention in this place, as they explain why the danger to the patient's life is greater and more imminent in proportion as the injury is nearer to the brain. According to my experience, where a considerable injury has been inflicted on the spinal cord in the lower part of the neck, or in the

neighbouring part of the back, of such a nature as to paralyse all the muscles of respiration, with the exception of the diaphragm, the patient rarely survives to the end of the fourth or fifth day, while in the majority of cases he dies at a still earlier period. The following case (to which I have already had occasion to refer) can scarcely be regarded as forming an exception to this general rule. In the patient to whom I allude, there was a fracture of the seventh cervical vertebra. The respiration was not affected in the first instance. On the twelfth day he suddenly began to breathe with difficulty, and by the diaphragm only: on the following day he died. On dissection it was ascertained that there was some displacement of the fractured bone, but not sufficient to occasion pressure on the spinal cord. An inch and a half of the cord was in a softened state, but the softening process had not proceeded so far towards dissolution, as in many other cases at the same period. I conclude that the cord had, in the first instance, undergone a very trifling degree of disorganisation, and that it was only as the effects of the concussion became more completely developed that the difficulty of respiration commenced.[1]

Cases of recovery even after what may be regarded as a severe injury of the spinal cord, are by no means uncommon. If it suffers from the effects of concussion, the recovery may be complete; the period of recovery varying from three weeks to twelve months or more. If the cord be lacerated, or much compressed by displaced bone, he may live, but without recovering from the paralysis. Under these circumstances life may be prolonged for an indefinite period. A man was admitted into St. George's Hospital in January, 1823, who had met with an injury in the preceding August, in consequence of a mass of chalk having fallen upon him while working in a chalk-pit. Mr. Hardwicke, of Epsom, being sent for, found the first lumbar projecting over the last dorsal. With some difficulty he reduced the displaced vertebra to its natural position, the reduction (as I was informed) taking place with a jerk or snap. At the time of his being received into the hospital, he had some power of using his lower limbs while in bed, but he

[1] In a case under the care of Mr. Green, in St. Thomas's Hospital, reported in the Medical Gazette, (vol. i. page 224,) in which there was a fracture and dislocation of the seventh cervical vertebra, the patient survived until the seventeenth day; but it was stated that the respiration was difficult, performed by the action of the diaphragm, with a trifling action of the intercostal muscles. The respiration became more difficult afterwards, but the exact period at which this change took place is not noticed. In another patient, also under the care of Mr. Green, (Medical Gazette, vol. vi. page 190,) in whom the third dorsal vertebra was the seat of the injury, there was, in the first instance, paralysis of the lower limbs, and it is represented that he breathed by the action of the diaphragm only: this last observation, however, was evidently erroneous, as he *breathed easily*, and as there was a slight motion of the ribs, attributed by the reporter to the expansion of the lungs. *Respiration by the diaphragm only is always difficult*, and the expansion of the lungs following the descent of the diaphragm is quite insufficient to account for the motion of the ribs. This patient recovered.

could neither walk nor stand, and he was unable to empty his bladder without the aid of the catheter. He remained nearly in the same state when he quitted the hospital, two months afterwards. When I last heard of him, after the lapse of two or three years more, he was still alive, but no material alteration had taken place in his symptoms.

SECTION III.

TREATMENT OF INJURIES OF THE SPINE.

In making this communication to the society, my principal object has been to analyse and arrange the pathological changes, and the symptoms to which injuries of the spine give rise. But I am unwilling to leave the enquiry so incomplete as it would be if I were to omit altogether the consideration of the surgical treatment which these accidents require, and I shall therefore, in conclusion, offer some brief observations in illustration of the last mentioned subject.

When a bone is dislocated, or when it is fractured and displaced, the first question which presents itself to the surgeon is, whether it ought to be restored to its natural situation? and if so, how is that to be accomplished?

Dislocations and fractures, with displacement of the cervical vertebræ, are not always immediately fatal; and I cannot say that no circumstances can exist which would justify the attempt to effect reduction in such cases: but it is evident, that if the attempt be made at all, it must be with the greatest caution, and Boyer describes a case in which a child died under it.

There can, however, be no doubt that, when the injury is in the lower part of the spine, the attempt to effect reduction may be not only made with impunity, but that it may be successful. In proof of this assertion, I may refer to a case, which I have already described, which occurred in the practice of Mr. Hardwicke, of Epsom; the patient being afterwards admitted into St. George's Hospital, labouring under paralysis of the lower limbs. In another case, to which also some allusion has been already made, and which occurred under my care in St. George's Hospital, there was a fracture, with great displacement of the third and fourth lumbar vertebræ. When the patient had recovered from the state of collapse which had followed the first shock of the accident, I endeavoured, by fixing the thorax and cautiously extending the pelvis, to restore the vertebræ to their proper place. The attempt was, in some degree, successful, and no ill effects of any kind resulted from it.

Some discussion has of late years been excited, in consequence of a proposal, which I believe originated with the late Mr. Henry

Cline, to apply the trephine in cases of fracture of the spine attended with depression of the bony ring of the vertebræ, with a view to the removal of the depression.

The question respecting such an operation seems to me to lie in a very small compass. If the whole or nearly the whole of a vertebra be driven forwards, the depression of the posterior part of it will of course occasion a diminution of the size of the spinal canal; but the removal of any portion of the vertebra which is accessible to an operation, will be of little avail, as the irregularity in the anterior part of the canal, made by the displacement of the body of the vertebra, must be the same after, as it was before, the operation.

If there be simply a fracture on each side of the spinous process, with a depression of the loose or intermediate portion of bone, of course there must be a corresponding diminution of the size of the vertebral canal; but as that canal is much larger than the spinal cord, which it contains, it does not follow that the spinal cord is really compressed, or that any material diminution of the symptoms would follow the elevation of the depression.

But let it be supposed that the spinal cord is really suffering from pressure: it has been already shown that a much less degree of violence than that which is necessary to occasion a fracture of the spine, may produce concussion, softening, and, ultimately, dissolution of the spinal cord, with a train of symptoms much worse than those which arise from simple pressure. Now, no operation can be of the smallest advantage in this respect: but, on the contrary, if it be necessary to apply the saw in the performance of it, the jar and disturbance of the parts which this must occasion is even likely to aggravate the mischief.

If these views be correct, it is evident that the cases, in which there are any reasonable grounds for the performance of the operation, must be of very rare occurrence, and that even under the most auspicious circumstances it must be doubtful whether it may not be productive of harm rather than of good to the patient.

Nor, as far as I am acquainted with the results, do the experiments, which have been hitherto made on the subject, lead to any more satisfactory conclusion. I am not aware that in any of the cases, in which it has been hitherto performed, the operation has proved the means of preserving the patient's life, or even of relieving any of the more important symptoms.

In the treatment of mechanical injuries generally, nothing is of so much importance as the maintenance of the injured parts in a state of complete repose, and it is not less indispensable for us to observe this rule in cases of injury of the spine, than it is on other occasions. With this view we lay the patient in a supine and horizontal posture on a mattrass. We can attain what is wanted in this respect by no other means: otherwise as good a reason might be offered for placing him on his face, with his spine uppermost, as for placing the head on a high pillow in a case of apoplexy or concussion of the brain.

I have shown that, in some cases, an injury of the spine is followed by inflammation of the membranes of the spinal cord, and there can be no doubt that it is then necessary to take blood from the arm, and even to repeat the bloodletting several times. The state of the pulse forms a sufficient indication to the practical surgeon of the necessity of such active treatment, and the appearances of the blood after it is drawn will assist his judgment in determining to what extent it should be carried. It is, however, if my experience has not much misled me, a great mistake to suppose that bloodletting is always proper. In the majority of cases the state of the pulse is such as actually to contra-indicate the abstraction of blood, and as I have already stated, the blood when drawn does not in general present those appearances which are supposed to mark the existence of inflammation. I have no reason to believe that bloodletting arrests the process of softening and dissolution of the spinal cord, and indeed I have usually found that the symptoms which mark the existence of these changes make a more rapid progress, in proportion as a larger quantity of blood is taken away. The weak and contracted pulse, the disposition to gangrene, the alkaline quality of the urine, the black and offensive alvine evacuations, and the brown or black fur on the surface of the tongue, would, under other circumstances, be regarded as proofs of depression and debility, indicating the use of stimulants rather than of what are called antiphlogistic remedies. From all that I have seen, I cannot doubt that much harm has arisen from the indiscriminate application of the practice, which is usually proper and necessary after injuries of the head, to cases of injury of the spine.

I have stated that a torpid state of the bowels is almost a constant result of these accidents, such as cannot be overcome except by the exhibition of powerful purgatives. I have generally found that the combination of ammonia facilitates their action, and that it will often enable them to produce the desired effect, when they would not have produced it otherwise. Attention to this part of the treatment is especially required when the evacuations are black and offensive. The retention of the unhealthy secretions, on which these qualities depend, cannot be otherwise than injurious to the general system.

The use of the catheter is necessary from the beginning, in all cases in which the lower part of the body is paralytic. It does not, however, prevent the urine becoming alkaline, nor the secretion of adhesive mucus from the lining membrane of the bladder. When these changes have taken place, the bladder should be emptied several times daily; and it may be advisable in some instances to inject tepid water into it, so as to prevent any portion of the mucus being retained in its cavity. The mucus is itself the product of inflammation; but on this, as on other occasions, it forms an irritating application to the parts with which it comes in contact, and thus tends to aggravate and increase the inflammation, on which the formation of it originated.

In the treatment of those cases of injury of the spinal cord, in

which gangrene takes place from pressure, we labour under greater difficulties than in ordinary cases of gangrene, owing to the very slight degree of pressure which is sufficient to produce these frightful consequences. All that it lies in our power to do is to cause the pressure to be diffused over as large a surface as possible, and to endeavour to increase the force of the heart's action by the prudent exhibition of stimulants.

OF

INFLAMMATION, CHRONIC DISEASE,

AND PERFORATIVE

ULCERATION OF THE CÆCUM,

AND OF THE

APPENDIX VERMIFORMIS CÆCI,

WITH

SYMPTOMATIC PERITONITIS AND FÆCAL ABSCESS.

BY JOHN BURNE, M. D.,

PHYSICIAN TO THE WESTMINSTER HOSPITAL.[1]

The diseases of the cæcum and of the appendix vermiformis, which form the subject of this paper, have received no separate consideration in the systematic works on practical medicine; nor have I met with any notice of them, except in the medical periodicals, in which detached cases have from time to time been published.

The diseases in question are, nevertheless, of frequent occurrence—are always dangerous, often fatal, and characterised by a train of symptoms so peculiar and marked as to render the recognition of them certain and not difficult.

So long as sixteen years ago, my attention was attracted to inflammation of the cæcum, by the case of a young lady who had become suddenly ill, with obstinate constipation of the bowels and vomiting, accompanied by febrile movement and local symptoms centred in the right ilio-inguinal region. The complaint was manifestly inflammatory; but the inflammation and febrile movement were developed less quickly than in an ordinary idiopathic inflammation, and it did not yield to treatment, nor yet run on to the rapid destruction of life, like an inflammation of the idiopathic kind. The case was obstinate, tedious, and dangerous, though eventually it terminated favourably. Reflecting on all these circumstances, and on the organisation of the region of the abdomen,

[1] Medico-Chirurgical Transactions, vol. xx., p. 200, Lond. 1837.

to which the symptoms were referred, I suspected the cæcum to be the seat of the disease, and that the inflammation arose from the accidental lodgment of some undigested substance—fruit probably, for it was in the autumn season, and the young lady had been eating freely of plums.

Other similar cases, occurring from time to time, confirmed my suspicions. I have seen in the whole not less than twenty, of which, some having recovered, others having terminated fatally and allowed opportunities of ascertaining by dissection the anatomical characters of the disease, materials have been furnished, from which I hope to be able to lay before the society a connected history of these affections. I am, moreover, anxious to direct the attention of the society and of the profession to these cæcum and appendix cases, because they are apt to be confounded with idiopathic abdominal inflammations, and to be treated as such, much to the injury, if not to the destruction, of the patients.

An idiopathic inflammation of the cæcum, from the ordinary general cause—exposure to the vicissitudes of the weather—has not fallen under my notice. The inflammation of which I am about to speak has in every case been symptomatic of some mechanical exciting cause—as the lodgment of undigested food, of fruit-stones, or of concretions, which the structure of cæcum and appendix favours; and hence the peculiar features of the disease.

The peculiarities of this inflammation, produced by such mechanical causes, are—the marked and fixed local signs; the invasion of them without any obvious cause, while the patient was in health; their gradual development; their obstinacy; the late supervention of the febrile movement, and its less degree in proportion to the local affection and suffering; and the less anxiety depicted in the countenance than is noticed in the idiopathic enteritic inflammation. These are the peculiarities.

In all the examples of inflammation of the cæcum which I have witnessed, the development of the symptoms has been in the following order. The first sign is a sense of uneasiness, which soon amounts to an aching pain, deep-seated in the right ilio-inguinal region, arising unexpectedly while the person was in health, and not preceded by rigor or exposure. This pain increases progressively for twelve or twenty-four hours, retains its character, is fixed and constant, never even remitting. Then supervene gradually tenderness, fulness, and tension of the whole ilio-inguinal region; the bowels are constipated and do not reply to medicine, and the patient grows sick and vomits. Some febrile movement now begins to manifest itself; the tongue becomes white and furred, the urine scanty; the appetite is gone; the pulse is frequent, tight, and sharp, with increased volume, but the stroke, though sharp, is not strong, nor is its impression on the finger decided—it is a pulse of irritation and inflammation combined; the patient lies on the back quite still, slightly inclined to the side affected, and the case presents a serious aspect.

This state of things will persist for several days, the pain remain-

ing of a very severe aching character. The fulness and tension of the part will increase, and extend to the other regions of the abdomen, which hitherto had been soft and not tender; and a sign will now be present, in an eminent degree, characteristic of this inflammation—it is an *exquisite tenderness* of the abdominal parietes covering the cæcum—a tenderness far exceeding that of an enteritis, even of a peritonitis; the patient will scarcely allow a finger to be laid upon the part; when you go to examine it he lays hold of your hand, and supplicates you in an earnest manner not to touch it, not to hurt him; he cannot bear the weight of the bedclothes upon it; the tenderness is as exquisite as in any case of acute rheumatism I ever saw. The constipation continues, but the vomiting does not become frequent and distressing, as in enteritis, nor does the face betray so soon the anxious aspect. Taking the case altogether, it is not such an imminent affair of life and death as an enteritis, though in the sequel it may prove equally fatal. Such are the signs of the symptomatic inflammation of the cæcum.

A favourable termination cannot be calculated upon till alvine evacuations have been procured, followed by a subsidence of the tenderness, tension, pain, and vomiting—all which can seldom be accomplished in less than seven or eight days. It may happen that the bowels cannot be brought to act, that the local signs do not give way, that the vomiting continues, and that the patient's strength declines seriously about the eighth day, especially if much blood has been abstracted; then he seems to sink rather from exhaustion of the powers of life than from the effect of the inflammation, and so he dies; or, if life is prolonged, there may be discovered about the tenth day a circumscribed emphysematous tumour, presenting in front in the right ilio-inguinal region, or posteriorly in the corresponding ilio-lumbar region, which will prove to be a fæcal abscess making its way to the surface of the body. Ulcerative perforation of the anterior or posterior part of the cæcum will have taken place; if of the anterior part, adhesion will have formed around the perforations, and thus the fæcal abscess will arrive at the circumference of the body without involving the peritoneum in a general inflammation; if of the posterior part, which has no peritoneal tunic, then the peritoneum will escape altogether, and the abscess tend upwards and backwards to the least resisting part of the lumbar parietes, which is at the outer edge of the quadratus lumborum muscle. This abscess may discharge itself, and the patient do well; or nature may be unequal to the task, and the patient sink exhausted.

The *diagnosis* of these cæcum cases is determined with precision by the seat of pain, the exquisite tenderness, and the tension—by the sudden invasion of the symptoms while the person was in health—by the local signs preceding the development of the febrile movement—by the degree of fever being less than in idiopathic inflammation, and by the less marked anxiety of the countenance. If any doubt remain, it will be dissipated by laying the

hand upon the part, when the circumscribed fulness and hardness in the region of the cæcum will give assurance of the seat and nature of the affection. A practitioner, who witnesses one of these cases for the first time, is satisfied it is not a common inflammation of the bowels, although he does not know its exact nature; he says the case is a curious one—he cannot make it out.

The *treatment* will not be difficult to determine, nor will it be often unsuccessful, if an accurate judgment is formed of the nature of the disease; but if the case should be mistaken for an idiopathic inflammation, and blood-letting be pursued actively in the expectation of subduing it, the patient will be hurried into danger, perhaps destroyed, by the remedy: for the cause of the inflammation being mechanical, the inflammation will persist until the cause is removed. It will be in vain, therefore, to expect to subdue the inflammation while the cause continues in operation; the abstraction of blood will at all times give a temporary relief, which allures practitioners on to persevere in its use to an unwarrantable and dangerous extent.

The indications in the treatment are, first, to moderate the inflammatory action to a degree consistent with the vitality of the organ affected, and with the life of the patient; secondly, to accomplish this with as little expense to the bodily powers as possible, in order that, should a fæcal abscess form, nature may be able to go through the tedious and difficult process which would be inevitable. Blood, then, should be drawn with discretion—the quantity should not be large at any time; and a repetition of the blood-letting should not be practised at so short an interval as in enteritis. Leeches are an excellent means of relieving this local affection, but even these should not be used in large numbers, nor too frequently. Eight or ten are as many as should be prescribed at one time, and they may be repeated daily at first, then every alternate day, with advantage. After the leeches, a warm oatmeal poultice should be carefully applied, not too thick nor too heavy, or the weight will not be borne. The leech-wounds should not be allowed to bleed more than one hour, or the patient may be drained of blood injuriously. The colon should be evacuated by a domestic enema, and aperient medicines be given with a view to dislodge the offending matter: an excellent form is one dram of the sulphate of soda in half an ounce of infusion of senna, to which may be added four drops of tincture of opium, to appease the stomach. The stomach, however, is not very apt to reject medicine, because the vomiting is not very urgent nor very frequent. Should the medicine not be retained, then infusion of senna, with tartarised soda, carbonate of soda, and tartaric acid, in a state of effervescence, will be preferable. Should this prove ineffectual, it may be proper, lastly, to resort to the compound extract of colocynth, calomel, and opium, in doses of six grains—two grains and one grain respectively every six hours. It rarely happens that blood is required to be drawn from a vein a second time, the local abstraction by leeches being more likely to relieve the local affection with less sacrifice of strength. A fomentation, dexterously and lightly applied and persevered in,

will prove a most valuable auxiliary; the hot-bath should not be proposed, any motion of the body being too painful to admit of its use. About the fifth or sixth day the bowels may begin to act, and lumpy pieces of undigested matter may or may not be recognised in the dejections, after which the symptoms will gradually subside and the patient recover.

Or the symptoms may persist, in which case it behoves the medical attendant to watch the intentions of nature, as a tumour may show itself in the ilio-inguinal or lumbar region, which should be encouraged to the surface by poultices; and the moment an emphysematous condition is distinguished, a free incision should be made, when a fetid gas with an offensive fluid will be discharged, and the cellular tissue discovered to be gangrenous. The tumour having been laid open, the patient's powers will require to be sustained by broths, arrow-root, weak brandy and water, ammonia, and, as soon as possible, by the decoction of bark—a form preferable at this time to the quinine; and an opiate in full dose should be given at night to procure rest. With this treatment patients will struggle through a long and most dangerous illness; but, I repeat, if blood has been abstracted copiously under the impression of the inflammation being idiopathic, the powers of life will be unequal to maturate the fæcal abscess, should one form, and the patient will fall inevitably a sacrifice to the error.

CASE I.

Inflammation of the cæcum, from the lodgment of some undigested matter probably.

Charles Neale, a healthy boy, twelve years of age, was seized about a fortnight before Christmas, 1828, on a Sunday morning, with an aching pain in the right ilio-inguinal region, which obliged him to leave the church, and kept him crying all day. The pain increased as the day advanced, did not remit nor recur in paroxysms, but remained fixed and constant. On the morrow he began to be feverish and to vomit. On the third day he was seen by a medical practitioner, who applied leeches to the seat of pain, and administered aperients; and on the fourth day, the symptoms increasing, blood was drawn to fifteen ounces—a large quantity for a boy twelve years of age; strong aperient medicines were continued. On the fifth day more leeches were applied, after which I was called in consultation. I found the boy lying on his back quite still, careful not to move his body lest it should aggravate the pain; the whole abdomen was tumid and tense, more particularly in the right ilio-inguinal region, where was a fixed aching pain, with tenderness so excessive that the slightest touch was reluctantly borne, while over the other regions of the abdomen the tenderness and pain were trifling: he was sick, and vomited from time to time; the tongue was foul and brownish; the pulse frequent and weak; and there had been no action of the bowels since the commencement of the illness—now six days—except from the colon by an enema on the second day. The case was recognised by me at once

as an inflammation of the cæcum, excited probably by the accidental lodgment of some undigested matter.

The boy was low and much reduced by the loss of blood and the length of suffering, yet the countenance did not betray anxiety in a degree relative to the other symptoms, which gave hope of a favourable issue. The exhausted state of the patient determined me to advise that all active measures should be laid aside; that no more blood should be drawn; that the strong purgatives should be discontinued, and gentle saline aperients administered in their stead; that the local symptoms should be soothed by fomentations; and that, relying upon these means, we should for a time wait the result.

On the day following (the seventh of the disease), I was gratified to find that he had slept through the night; that the bowels had acted several times, and some hard lumpy matter been discharged; that the tenderness, pain, and tension had diminished, and that thus all obstacle to recovery had been removed. From this period the boy's health was quickly and perfectly re-established.

The above case is illustrative of a fact I have often observed—namely, that obstructions of the bowels which had resisted strong purgatives have eventually yielded to gentle saline aperients. In the case of an elderly lady, labouring under an alarming obstruction of the bowels from incarcerated umbilical hernia, which persisted after the hernia had been liberated, strong purgatives not only failed to relieve the obstruction, but kept up the vomiting; while the stomach became quiet, and the bowels acted, under the use of mild saline aperients.

CASE II.

Inflammation of the cæcum from the lodgment of a piece of apple.

For this case I am indebted to my friend Mr. Selwyn, of Ledbury. "I have met," he says, "with some very obscure cases connected with inflammation about the cæcum. I was called, on the 22d of February, 1827, to Mr. Frank, a farmer, whom I found complaining of extreme pain and tenderness in the situation of the cæcum; and, on very careful examination, I discovered a hard body which I conceived like something lodged in the blind extremity of that gut. The bowels had been constipated for a week. I treated the case as inflammation; bled him six times in four days, and applied leeches in large quantities. The case terminated in a very satisfactory manner, although at one period I could not tell which way the balance would go, when the bowels began to act copiously, and he voided what I fancied was the foreign substance, which the nurse had unfortunately thrown away, so that I did not see it, but she and a friend describe it as being a large piece of undigested apple; and on questioning Mr. F., he recollected having swallowed accidentally a piece of a very hard apple a fortnight before."

It is a fact, established by pathological anatomy, that those parts

of the intestinal canal where its dimensions vary, and its organisation changes, are particularly liable to be affected with chronic disease. The cæcum is one of these parts; and, in addition to the acute inflammation which has been described, it is not unfrequently the seat of a sub-acute chronic inflammation or pathological congestion, which induces thickening of its tissues and contraction of its natural capacity, impairing thus the organisation and function of the gut, so as to render the action of the bowels irregular and difficult, and eventually to determine a complete and fatal obstruction. This diseased condition of the cæcum may exist for several months, or even years, accompanied always with alvine difficulty and sympathetic disturbance of the stomach, of the same character exactly as that which attends stricture of any part of the alimentary canal. The sufferings are similar, the health declines, the body wastes away, and patients die, worn down by sickness, inanition, and the dreadful spasmodic pains arising from the violent efforts of the bowel to overcome the obstruction. When emaciation has arrived at a considerable degree, the powerful peristaltic action may be felt, even seen, heaving the abdominal parietes in tracts corresponding with the convolutions of the intestines.

CASE III.

Chronic thickening and contraction of the cæcum; obstruction, more or less, for several months; sickness, emaciation, death.

Ann Morris, aged thirty-one, was admitted into Guy's Hospital in August, 1830, in a very emaciated state from an illness which had afflicted her many months, and of which the prominent symptoms were an obstinately constipated state of the bowels, together with sickness so frequent that the greater part of her food was cast up again. She complained of pains in the belly like the throes of labour, and in the right ilio-inguinal region was discoverable a small hard tumour, which I concluded to be the cæcum diseased; the abdomen was tumid and flatulent, and a strong vermicular motion within the belly was observable, corresponding exactly with the peristaltic action of the intestine; besides which, the distended convolutions of the small intestines could also be accurately traced by the undulating elevations of the abdominal parietes.

Various means—as magnesia, soda, leeches, lime-water, opium, hyosciamus, and clysters—were employed by her physician without benefit, and in the course of a fortnight she sunk and died.

On *dissection*, the cæcum was found to be thickened and much contracted; the ileon distended with flatus and loaded with fæculent matter, while the colon was empty and contracted. The sigmoid flexure of the colon was stretching over to the right side, in contact with, and adhering to, the cæcum and ileon by fibrine recently effused: a singular circumstance, leading to the belief that nature was endeavouring to effect a communication with some part of the intestine above the obstruction, which would have restored the channel of the bowel, and perhaps have saved the life of the

patient, if her powers had not sunk before the completion of the process.

CASE IV.

Disease of the cæcum, with organised bands stretching across its cavity in different directions, forming a net-work, and causing obstruction and death—Vast accumulation of fæces above the obstruction, ileus, and sero-enteritis.

The subject of this case was Mary Ann White, a patient of the Public Dispensary in the year 1830, twelve years of age, of small stature, her frame of body not exceeding that of a girl eight years old, though in no way deformed, while her face was fully developed. Ill health, it was said, had impeded her growth for several years.

The history of her complaints, as far as I could ascertain, was, that she had suffered for two or three years from large tumid abdomen, with a very irregular and difficult state of the bowels, flatulence, spasmodic pains, loss of appetite, continued emaciation, and frequent attacks of sickness and vomiting. The dejections were generally scanty, soft, and very offensive—characters indicative of obstruction from organic disease. A fortnight preceding her death, she was seized with unusually severe pain in the belly, followed quickly by sickness and vomiting; the abdomen was immensely distended, and very tender all over, and the dejections consisted only of mucus without any trace of fæculent matter. The vomiting continued till her death, prior to which, for some days, the matter thrown up was distinctly fæculent, constituting the ileus of nosologists.

Sectio cadaveris, fourteen hours after death. The body was emaciated to the greatest degree; the abdomen enormously distended, and its surface varied by furrows and bulgings, caused by and corresponding with the convolutions of the small intestines. The abdomen opened, the small intestines were seen distended excessively, being three or four inches in diameter, of a mottled livid red, and agglutinated together by soft albuminous matter. Tracing the intestinal canal, the seat of the obstruction was found at the cæcum, to which point the distension of the bowels continued, while beyond it the colon was empty, contracted, and sound.

The cæcum, when removed and examined, proved to be contracted and thickened; its tunics being blended together, and transformed into a dense, opaque, white, unyielding, gristly substance; and interiorly were discovered numerous organised bands covered with a smooth shining membrane, stretching across the channel of the gut from side to side, in various directions, forming an irregular coarse net-work. In this contracted cæcum and net-work, a complete obstruction had been formed by fæculent matter, dry and friable, plugging up the channel in a manner which nothing could have removed. Leading to this part was the ileon, filled with an amazing quantity of soft, yellow, homogeneous, fæculent matter—the bowel itself having all its tunics confounded together, and converted into a dense strong tissue, a line in thickness, resembling thick wet parchment: all trace of villous structure, or valvulæ

conniventes, having disappeared. The colon was healthy; the uterus[1] not larger than the half of a small filbert.

The nature of the organic changes, just described, points to the cæcum as the cause of all the complaints; but how disease in it was originally excited, it is impossible to determine.

In chronic obstruction of the bowels from stricture, or other disease, it is not uncommon for attacks of peritoneal inflammation to supervene, excited by the distension, as in the case above described. Several attacks will succeed each other at longer or shorter intervals, marked by the characteristic signs.

And I have noticed that the fæculent matter, however large the quantity, above an obstruction, is invariably soft and homogeneous —a state strongly contrasted with the hard, scybalous state of fæculent matter detained in the bowel by ordinary constipation. It would seem to be a provision of nature to preserve the fæculent matter, above an obstruction, in a soft state favourable to its passage through it; and remarkable it is, that immediately any portion of this has passed the obstruction, its liquid parts are absorbed, and it becomes hard and friable as in habitual constipation—changes which, if they took place in the fæculent matter above a stricture, would be productive of total obstruction and death in every case.

CASE V.

Fæcal abscess in the right inguinal and iliac regions from ulcerative perforation of the cæcum.

Ann Box, aged fourteen, admitted into Guy's Hospital on Monday, the 2d of November, under the care of Mr. Key. She was naturally healthy, and reported that as far back as Christmas last—a period of ten months—she received a kick from her mother-in-law, in the right groin, which left a considerable contusion for the space of a week, and then got well. In May following, she was seized rather suddenly with pain, deep-seated in the right ilio-inguinal region, on which supervened tenderness, obstinate constipation, and vomiting. Her medical attendant, Mr. Hickman, bled her; and, having also leeched and blistered the part, she seemed to be getting well, when a swelling took place, which threatened to be an abscess. By the aid of leeches, this subsided very much; yet a tenderness remained, and the patient could feel a small lump inside, which was always so sore that she guarded it with her hand if any one came too near her. In this state things continued for five months —viz., to the end of October; on the 24th day of which, on a Saturday, she was seized in a moment with a violent pain in the region of the sore lump, followed quickly by shivering, vomiting, and purging. Mr. Hickman saw her again, and applied leeches,

[1] I have seen many instances in which the development of the sexual organs has been interrupted by organic disease of some viscus. If the individuals have lived, the menstrua have either not appeared, or have been scanty, pale, and irregular. Visceral disease may, therefore, be ranked as one cause of amenorrhœa.

which gave ease; but the part enlarged gradually, and became red; the sickness continued; shivering recurred frequently; and the bowels, having kept open till the 31st, became then obstinate.

On the 1st of November, she was seen by Mr. Key; the swelling had assumed the character of a deep-seated abscess, which he punctured, and there came away a few ounces of thick bloody matter, of a very offensive fæcal odour. Next day she was admitted into the hospital, and I had an opportunity of examining the part, and witnessing the progress of the case. The puncture was situated midway between the pubes and anterior superior spinous process of the ileum; and from it was discharged a great quantity of dark, thick, dirty matter with fetid gas. The right inguinal and iliac regions were full and hard, the hardness and fulness extending upwards and backwards, in the course of the spine of the ileum to the lumbar region, where the skin was red, as if the abscess was pointing in this direction also; the part was so tender that she would not allow any thing to touch it; she felt the belly tight, as if it was going to burst, and hot as if a ball of burning coals was in it. The bowels continued obstinately confined for four days, and then acted, much to the relief of the violent sufferings described.

On the 5th, a second opening was made near the anterior superior spinous process of the ileum, the discharge from which reduced the swelling and inflammation in the lumbar region.

On the 7th, a third opening was made higher up and more posteriorly, through which escaped matter and sloughing muscle and tendon, and sloughs of the same tissues have been drawn out of the first and second openings also. A probe, introduced through the first opening, passed more than four inches in the direction of the cæcum. Since the free incisions were made, and the sloughs have come away, the patient has been much easier, and the redness and tumefaction in the lumbar region have disappeared; and, although she is very weak and low, she sleeps tranquilly, her bowels act daily, and the appetite and general health improve. More sloughing tendon, two inches long, was removed on the 10th. The wounds were now granulating healthily, and from this period all pain ceased, and she regained her strength and health so rapidly as to be able to leave the hospital on the 24th, quite well.

Of the Ulcerative Perforation of the Appendix Vermiformis Cæci, and consequent peritonitis and gangrenous or fæcal abscess.

The appendix vermiformis, with which the cæcum is surmounted, is, like the cæcum itself, liable to be the seat of disease; the character of which, however, differs materially, owing to the peculiar conformation and situation of the appendix.

The conformation and situation vary much in different individuals—a fact not noticed by anatomists, but which I have found

to influence the phenomena and nature of its diseases very considerably. The conformation of the appendix is generally described as flexuous, and its situation as depending into the pelvis; but by some the situation is not noticed,[1] further than that the appendix arises from the cæcum and is bound down to it on the right by a fold of peritoneum, the meso-appendix; whereas the appendix is more frequently situated on the outer edge of the psoas magnus, on the facia iliaca, snugly curled up beneath the cæcum, and concealed by it—a fact which I have verified by many dissections, and one of great importance to the pathologist, as will be seen. In the event of a perforative ulceration of the appendix, and a consequent peritonitis or fæcal abscess, the parts involved will differ entirely, according to the situation of the appendix. If it should happen to depend into the pelvis, then the pelvic viscera will be implicated; if it should happen to be situated on the iliac fascia and underneath the cæcum, then the belly of the iliacus internus and the neighbouring adipose cellular tissue will be involved, and the course of the abscess be determined accordingly: so important is the relative anatomy of even inconsiderable organs to the physician. In a case to be related where the abscess pointed in the loins, the whole belly of the iliacus internus was in a state of gangrene, as was also the mass of adipose tissue in the lumbar region; the abscess was here making its way to the outer margin of the quadratus lumborum muscle—the part where the parietes offer least resistance to its progress.

The free communication of the appendix with the cæcum, by an open mouth, exposes it to the accidental intrusion of small portions of the residuary alimentary matter, or of any other substance which may happen to be passing through the cæcum; and this matter or substance, once within the appendix, can only escape by a retrograde course, the appendix being blind or closed at its other extremity. Whether the appendix has the power of expelling any substance from its canal, it is difficult to say; but should the matter or substance which has accidentally passed into the appendix be large in proportion to the calibre of this gut, it would be impacted and fixed in the canal; would become a source of irritation and of ulceration, perhaps of perforation, and all its disastrous consequences. It is far from an uncommon occurrence to meet with small bodies—as raisin stones—in the appendix, without any mischief having arisen from their presence; because the organic sensibility of its mucous lining, in common with the mucous lining of the intestinal canal, is adapted to the presence of foreign matter. These small bodies may, however, if they remain long, induce ulceration of the mucous membrane—a lesion often found—without producing more inconvenience than is caused by a superficial ulceration of any other part of the alimentary canal; but if a substance larger than the canal of the appendix—as a cherry-stone, or an intestinal concretion—happen to be forced into it and become

[1] Winslow, Cloquet.

impacted, then not only does it produce ulceration of the mucous membrane, but of all the other tunics until it reaches the peritoneum, which, being thus deprived of its means of nutrition, dies, sloughs, then bursts, and a perforation is effected. Now, so long as the ulceration is limited to the mucous membrane, it is of little consequence; but immediately that the peritoneum is perforated inflammation ensues, for the organic sensibility of the peritoneum will not suffer the presence of any foreign substance. Inflammation, therefore, is lighted up, and may spread with rapidity over the whole continuous surface of this membrane, constituting an universal peritonitis; or the inflammation may be limited to the vicinity of the perforation—may be circumscribed, and an abscess form: the danger in either case is considerable; indeed, the universal peritonitis will prove fatal to a certainty; and so much disturbance, both of the constitution and of the functions of the alimentary canal, arises from the abscess, as always to endanger, sometimes to destroy life. The abscess may nevertheless come forward, burst, discharge itself, and the patient recover; or it may remain circumscribed and stationary, forming a deep-seated painful tumour in the region of the cæcum, which, by its proximity to this gut, may produce continued obstinate constipation of the large intestines, a sympathetic disturbance of the stomach and of the whole system, and thus gradually wear down and exhaust the unfortunate patient.[1]

CASE VI.

Sloughing of the extremity of the appendix vermiformis cæci—Circumscribed abscess—Death.

Walking one afternoon with a medical friend, he requested me to call and see a case of a very obscure nature, under the care of himself and an hospital physician, which he feared was going on to a fatal termination. The patient, a baronet's coachman, fifty-seven years of age, had become affected three weeks previously with febrile movement, succeeded by vomiting and by constipation, which required the use of the strongest cathartics to procure dejections. In the course of the second week he complained of pain in the ilio-inguinal region, for which leeches and a blister had been applied; and about this period there occurred also retention of urine. When I examined him, he was lying on his back in bed, much exhausted, with the tongue beginning to get brown and dry, and the pulse frequent and weak. He vomited frequently, and the bowels were so obstinate that no aperient but croton oil would act upon them. He still laboured under retention of urine; the belly was full and tense; and, in examining the region of the cæcum, to which he referred as the seat of pain, I discovered immediately a circumscribed, hard, deep-seated tumour, the size of a small orange, and gave it as my opinion that the disease was situated in the

[1] Since writing the above, a case has come under my care in which a similar tumour burst into the bowel, discharged itself through this channel, and the patient recovered perfectly.

cæcum or appendix. After this he survived not more than eight days—the vomiting, obstinate constipation, pain, and retention of urine, having continued to the last.

Sectio cadaveris.—The right inferior portion of the omentum was adherent to the cæcum by fibrine, tender and recently effused; the omentum being carefully detached, there came into view a bag of matter or circumscribed abscess, which was found to be situated between the cæcum and iliacus internus muscle. The whole of the cæcum, as well as part of the colon and of the cæcal extremity of the ileon, were removed and carefully examined, when it was seen that the channel of the bowel was perfect, though the cæcum was contracted, but the appendix was only one inch long, and terminated openly in the abscess, its blind extremity having sloughed away. It is, therefore, more than probable that some small substance had become impacted in the appendix, and caused an ulcerative perforation and destruction of its blind extremity, followed by a circumscribed inflammation and abscess, of which the precise locality was determined by the situation of the appendix underneath the cæcum, on the outside of the psoas, and upon the iliacus internus muscles.

The immediate contiguity of this abscess to the cæcum was evidently the cause of the obstinate state of the bowels, and of the irritability of the stomach, which exhausted and destroyed the patient.

CASE VII.

Ulcerative perforation of the appendix vermiformis, from an intestinal concretion—Consequent peritonitis and death.

The subject of this case was a boy twelve years old, under the care of Mr. Wray, from whom I had the following particulars:—The boy was attacked with a deep-seated, fixed pain in the right ilio-inguinal region, followed in a short time by febrile movement, vomiting, and a tender and tense state of the whole abdomen—symptoms characteristic of a severe and sudden peritonitis. The inflammation admitted of no relief; its course was exceedingly rapid, and carried off the boy in two or three days. The bowels were readily acted upon by medicine from the commencement of the illness.

Sectio cadaveris.—Layers of recently effused fibrine were spread over the different parts of the intestines, the peritoneal tunic of which, as also of the abdominal parietes, were very vascular. The appendix vermiformis was found perforated about three quarters of an inch from the cæcum, by an ulcer having an irregular sloughing edge, in which perforation was lodged a concretion about the size of a cherry-stone. The appendix being slit open, another ulcer was discovered in the mucous lining, which was vascular, soft, and swollen, and the mouths of its mucous follicles were very distinct. In the interior of the cæcum was a portion of its mucous membrane the size of half a crown, pulpy and brownish gray; and in

the ileum, about half an inch from the valve, was a thickened spongy portion of mucous membrane thickly studded with muciparous follicles, the ducts of which were evident; and the mesenteric glands in the vicinity of the cæcum were enlarged and vascular.

CASE VIII.

Fæcal abscess in the right iliac region—Sloughing of the blind extremity of the appendix vermiformis cæci—Gangrene of the iliacus internus, and of the outer fibres of the psoas magnus muscles and of the sub-peritoneal adipose cellular tissue in the right lumbar region.

Mary Ann Hodges, aged nineteen, of good frame and stature, was admitted into Guy's Hospital the 10th of January, 1828, under the care of the senior physician. Four days previously she was attacked with severe pain in the right inguinal region, for which she had been bled three times, and leeched and blistered. On admission, being the fifth day of her illness, she complained of great pain and exquisite tenderness in the region of the cæcum, extending upwards to the right hypochondrium: she was vomiting a yellow bilious fluid; the pulse was frequent and small; the skin hot and dry; thirst great; and she lay on the back, inclined to the right side, with the legs drawn up—any change from which position aggravated the pain, and gave it a dragging character.

Twenty leeches to the seat of pain; one grain of calomel, and one of opium, every four hours; a castor oil injection. Some slight relief followed the use of the leeches, and the bowels acted several times. Next day, the sixth, twenty more leeches. On the seventh day the pain was unabated, and tenderness continued excessive; tongue glazed at tip; some sordes on the teeth; pulse 112. She was now bled to ten ounces, and ordered to take calomel, two grains —opium, one grain—tartarised antimony, half a grain—every four hours. The pain not having been relieved by the blood-letting, and the pulse having improved, it was repeated in two hours to twenty ounces, after which the pulse became very soft. The night was restless, and on the eighth day the symptoms were not mitigated, and the tenderness and fulness were extending to the left side of the abdomen; she was sick from the antimony. On the ninth day the bowels still acted, but, instead of amendment, hiccough came on, and a greater fulness and hardness in the iliac region was manifest. Blood was again drawn to ten ounces, and the opium and calomel continued. On the tenth day the features were sunk, and the face exsanguine; and now was discovered an obscure fluctuation in the right inguinal and iliac regions, which having become more distinct on the tenth day, an incision of two inches was made through the parietes of the abdomen in the ilio-lumbar region, a little above the spine of the ileum, and dissection continued down to the peritoneum, which was near two inches from the surface: a quantity of highly fetid gas and thin fetid pus escaped through the opening, and the finger introduced passed into a boggy cavity. The operation was followed by relief to the local

sufferings, but the powers of life were exhausted, and death ensued on the following morning.

Sectio cadaveris, five hours after death.—The viscera which came into view on the parietes of the abdomen being partially reflected were healthy, but exsanguinous. The parietes of the right side being reflected further, the right inferior portion of the omentum was seen agglutinated to the peritoneum and cæcum; and the agglutinated omentum being detached, discovered a shut cavity filled with offensive dark thick fluid, not of uniform consistence or colour, some portions being of a dingy yellow fæculent character. This fluid, amounting to half a pint, being sponged away, exposed the extent and situation of the cavity, which was in the peritoneum of the right iliac and lumbar regions, walled in by the adhesions of the omentum, peritoneum, cæcum, colon, and right lobe of the liver. The peritoneum forming the posterior boundary of the cavity was puffed up and black, and, being cut through, showed the whole of the loose adipose cellular tissue of the lumbar region in a state of gangrene—the gangrene extending to the same tissue behind the kidney, as also behind the cæcum and ascending colon, and contained some putrid fluid and gas which had puffed up the peritoneum, as seen before it was cut through. The outer portion of the psoas magnus, and the greater part of the iliacus internus, was gangrenous also. The colon and cæcum were healthy, but the blind extremity of the appendix vermiformis had sloughed away, the remaining part being pervious and opening into the cavity of the abscess, and containing thin, yellow, fæculent matter, such as was found in the contents of the abscess. The same kind of matter was also in the cæcum. The kidneys normal, as also the viscera of the pelvis. The liver sound, except the margin of the right lobe, which was adherent to the peritoneum.

The muscles of the abdomen in the right iliac and lumbar regions were separated from each other to the extent of a quarter of an inch, by soft albuminous matter recently effused, which caused a preternatural thickness of the abdominal parietes; and, with the abscess beneath, produced the fulness, hardness, and indistinct fluctuations during life. It was through these thickened parietes the incision was made, which had penetrated into the gangrenous mass of adipose cellular tissue, and reached the peritoneum, but had not perforated through it into the abscess.

From the evidence afforded by this dissection, it must be concluded that the sloughing of the appendix, from some cause, had allowed of the escape of the yellow fæculent matter of the cæcum into the peritoneum, which had excited a circumscribed inflammation, walling in the effused matter, and thus producing fæcal abscess; that this abscess caused the gangrene of the psoas and iliacus internus muscles, and of the adipose cellular tissue, and death of the lumbar peritoneum, which, from the dense character of its organisation, had not sloughed or burst. The abscess was making its way to the surface of the body in the ilio-lumbar region, where, by the aid of the incision, it would have discharged itself

as well as all the sloughing tissues, and the patient might have recovered, if the powers of life had been equal to the process; but these had been exhausted by the repeated abstractions of blood.

The practical inference, therefore, is, that had the case been understood, and the powers of the patient been husbanded, instead of exhausted by a treatment adapted only to an idiopathic abdominal inflammation, her life might probably have been preserved.

RESEARCHES

ON

EMPHYSEMA OF THE LUNGS.

BY M. LOUIS,

PHYSICIAN OF LA PITIE, MEMBER OF THE ROYAL ACADEMY OF MEDICINE, ETC. ETC.

TRANSLATED BY T. STEWARDSON, JR., M. D.

Physician to the Blockley Hospital; Recording Secretary of the Academy of Natural Sciences; Fellow of the College of Physicians, and Member of the Medical Society of Philadelphia, of the Societé Medicale d'Observation of Paris, &c.

TRANSLATOR'S PREFACE.

The work of which the following is a translation forms a part of the first volume of the Memoirs of the Medical Society of Observation of Paris, recently published. It contains much new and valuable information in relation to Emphysema, derived from the analysis of a large number of cases of the disease observed chiefly in the wards of M. Louis. The translator being engaged in his studies, and in the habit of making daily visits to the wards of La Pitié, at the time that the author was occupied in his researches upon this subject, had thus an opportunity of observing for himself a portion of the patients referred to, and can bear testimony to the care and attention which were bestowed upon their examination. The cases are analysed numerically, and the conclusions of the author are limited to such as are directly deducible from the facts before him. The whole essay, indeed, is a beautiful specimen of that strict analysis and rigid induction based upon *recorded data*, from which medicine is beginning to derive so much advantage. To extend the sphere of its usefulness, and thus assist in promoting the more general adoption of the same philosophic method of investigation on which it has been conducted, is the principal object of the

TRANSLATOR.

Philadelphia, Jan. 13*th*, 1838.

Emphysema of the lungs, or dilatation of the pulmonary vesicles, to which the attention of observers was first drawn by Laennec, has been almost entirely neglected since the time of that illustrious physician; nevertheless, it is one of the most frequent and remarkable affections to be found in the whole catalogue of nosology, and one whose history most imperatively demands for its elucidation a large number of new and well observed facts. It is true that Laennec has described the greater part of the symptoms by which the

disease may be recognised, but there is a wide interval between the mere diagnosis of any given disease and its complete history; and it is this interval which the successors of Laennec are called upon to fill up. It is not for me to enjoy the satisfaction of accomplishing the task; I only hope that the analysis of the facts which I am about to give will not be found altogether useless, and that it will add something to our stock of positive knowledge in relation to emphysema.

The facts alluded to are contained in the histories of ninety cases, forty-two of which proved fatal, whilst the subjects of the others left the hospital more or less relieved. Of the last mentioned cases, nineteen were collected by J. Jackson; four of those first noticed also belong to him, and moreover are peculiarly interesting, as will soon be apparent, on account of the numerous details which they present, as well as their great exactness. The remainder were observed by myself, for the most part within two years—that is, since the period at which I commenced to give more attention to this subject than I had previously done. I may add that twenty-three of the patients who died were carried off by the cholera; and that although it was impossible to obtain an account of the symptoms which they previously had laboured under as a consequence of emphysema, their history is nevertheless of considerable value.

I shall present but few cases in detail, enough only to enable the reader to be sure of the diagnosis of the affection, and at the same time make him perfectly acquainted with the character of the cases which I have analysed. I shall commence by giving a general description of the affection, after which I shall present a more detailed account of the various lesions, and give a separate consideration to each symptom. Finally, I shall undertake to investigate certain questions which the study of the facts laid before the reader will warrant me in entering upon.

CHAPTER I.

GENERAL DESCRIPTION.

Both the age and constitution of the patients affected with emphysema were very various. In some the constitution was strong, either originally or at the time when they were under observation; in others, it was more or less delicate. A portion of them were under twenty years of age, whilst others were seventy and upwards; so that the mean age of those who left the hospital more or less relieved was forty years, and of those who died, sixty. This fact is very remarkable, and shows at the outset how exceedingly slow is the progress of the affection.

The period of life at which it commenced was very different in different individuals; oftentimes it showed itself in early youth, and even in infancy, by a greater or less amount of dyspnœa, which

prevented the patients from running as rapidly as their companions, and from entering fully into their sports, as they soon found themselves out of breath. Frequently, also, this symptom did not make its appearance till much later in life. Sometimes it existed alone; at others, it was accompanied with cough. Once established, it persisted for several years—in some cases without variation, in others more or less rapidly increasing in severity. In many patients, it became at intervals suddenly and violently augmented, so that, if they happened to be lying down, they were obliged immediately to place themselves in an upright position, or even fly to the window for breath. These paroxysms, which sometimes came on without any appreciable cause, were commonly induced by an attack of acute pulmonary catarrh, either alone or supervening upon a chronic catarrh. They continued for one or more days with greater or less severity, becoming less violent with the diminution of the catarrh, and finally disappearing along with it. These violent paroxysms rarely showed themselves at the commencement of the disease, but appeared to be inseparably connected with it at a certain period in its course, and augmented generally both in force and frequency as the patients advanced in life. They were commonly accompanied by palpitations, which became permanent in a number of instances. Under these circumstances, the lower extremities were frequently the seat of œdema, which disappeared as the paroxysms of dyspnœa passed off.

To these general or rational symptoms were joined local signs, the presence of which can leave no doubt about the diagnosis of the disease, of which the preceding account is already sufficiently characteristic. The conformation of the chest was altered; the thorax was more prominent upon one side than the other, and over a surface of variable extent. Over or around this prominence, the percussion was more sonorous than natural, and the respiratory murmur, on the contrary, more feeble. Frequently, also, a sibilant râle was heard in other parts of the chest, accompanied by great feebleness of the respiratory sound. The pulsations of the heart, though sometimes accompanied by impulsion, were often very feeble, especially when the prominence spoken of happened to be in the præcordial region. The general strength of the body was in an inverse proportion to the dyspnœa. When the latter was trifling, the former was but little diminished, so that the individuals affected could perform their usual labours, which were sometimes very severe, although with a little less energy than when in perfect health. When the dyspnœa was more severe, the strength diminished very much; so that, at a certain period of the disease, patients who had neither palpitations nor any symptom of affection of the heart, nor œdema, nor fever, and sometimes not even a pulmonary catarrh, or at most a very slight one, were obliged, merely on account of the severity of the dyspnœa, to avoid every species of labour; or if they did undertake any light work, it was but for a short time, when a paroxysm would come on and oblige them to enter a hospital.

Persons affected with emphysema lost neither flesh nor appetite, except during a paroxysm of dyspnœa, which was accompanied with intense pulmonary catarrh and fever. In the intervals of these attacks, in fact, the patients (I speak of those who were under 60 years of age) presented a healthy appearance, having neither heat of skin, frequent pulse, nor unnatural thirst. Moreover, this disease, however aggravated, was never alone the cause of death, which was mainly owing to some other affection subsequently developed; and, upon opening the body, the lungs were uniformly found to be the seat of a peculiar lesion. They did not collapse; they were more voluminous than natural, and sometimes overlapped one another at their free borders. This border was thicker than common, and when compressed between the fingers, in order to drive out the air which it contained, its tissue was found to be evidently more dense and not as thin as that of a healthy lung under similar circumstances. The pulmonary vesicles were dilated, and this dilatation, which existed in various degrees in the same lung, was sometimes so considerable that the vesicles, or at least some of them, were as large as a small pea. Occasionally, also, they were ruptured and looked like appendages to the border of the lungs. The bronchial tubes were not often dilated. The heart was hypertrophied and dilated in some cases.

The other lesions were in no respect different from those which are found in patients who die of any other disease.

We will now enter upon a detailed examination, commencing with the study of the lungs.

ARTICLE FIRST.

ANATOMICAL DETAILS.

§ I. *Of the Lungs.*—The lungs in those parts where the cells were dilated, yielded less easily to pressure than when in a healthy state, and, as above remarked, their tissue, after being deprived of air, was thicker than usual; that is to say, it was hypertrophied. But where was the precise seat of the hypertrophy? I answer, in the vesicles; at least every thing conspires to render this supposition probable; for it is a law of our economy that membranous tissues become thicker whenever they are distended from any cause. Thus the œsophagus is thickened in cancer of the cardia, the stomach in that of the pylorus, the intestines owing to any obstacle to the free course of the matter which they contain, the walls of the heart in dilatation of that organ, the ureters in organic diseases of the uterus, which contract the orifices by which they open into the bladder, &c. Besides, direct proof of the fact can be obtained by drying two lungs previously inflated, the one healthy, the other emphysematous, and then making very smooth sections of each in corresponding parts, when it will be clearly seen that the walls of the dilated cells are the thickest.

The precise determination of the above fact is especially important

on account of the influence which the thickening of the walls of the vesicles must have upon the phenomena of respiration. For whatever be the exact nature of the reciprocal action which the air and blood exert upon one another during respiration, it is clear that such an action really takes place, and must necessarily be affected by every thing which affects the dimensions of the pulmonary vesicles, and especially the thickness of their walls. Besides, it seems to me that this is almost the only way in which we can account for the dyspnœa in cases where there is no fever, where the blood circulates freely, and where the lungs appear to contain more air than natural. Supposing this view to be correct, we can conceive that emphysema might exist, even to a great extent, without at the same time causing much dyspnœa, provided in opposition to the general law the dilated vesicles were either not at all or very slightly thickened, as was perhaps the case in the subject of case No. 1. In like manner we can understand how hypertrophy of the vesicles, without any dilatation, should give rise to several of the symptoms of emphysema, or at any rate to oppression. But these views cannot be regarded as scientific truths, until they shall have been confirmed by experience. Let us regard them at present, then, as mere probabilities, and continue the detailed examination of the facts before us. The emphysema varied very much in extent in the different cases. Sometimes it was general and occupied the whole of both lungs, at others it occupied only one of these organs, and sometimes merely a portion of one or other of them. The distribution of these different cases was as follows:—

1. *In the nineteen cases who did not die of cholera.*

Emphysema general throughout both lungs, 12 cases.
" general throughout left lung, 2 cases.
" general throughout right lung, 1 case.
" of left superior lobe, 2 cases.
" of right inferior lobe, 2 cases.
" of the two middle fifths of right lung, 1 case.

2. *In the twenty-three cases who died of cholera.*

Emphysema general throughout both lungs, 6 cases.
" general throughout left lung, 3 cases.
" general throughout right lung, 1 case.
" of left superior lobe, 7 cases.
" of right superior lobe, 7 cases.
" of left inferior lobe, 1 case.
" of right inferior lobe, 2 cases.
" of middle and superior right lobes, 3 cases.

Hence it appears that in the first series, or those who did not die of cholera, general emphysema was found in two thirds of the cases; whilst among those belonging to the second series, it existed in only a fourth part.

This difference should not be regarded as accidental, for the mean age of the patients composing the first group was 60 years; that of the second, 50 years. Hence it appears that the extent of the affection is proportionate to its duration, and moreover that it is not general at the commencement, at least in a majority of cases. Besides, by uniting the two groups above mentioned, and then dividing them according as the individuals were above or below 50 years of age, we find that general emphysema occurred in fifteen out of thirty-one cases, who were over 50 years of age, while it existed in only three out of twelve of those who were less advanced, or who were 29, 38 and 40 years old respectively.

Further, as is evident from the preceding statements, the relative frequency of emphysema upon the right and left side is very nearly the same. In fact, general emphysema existed

On the left side in 23 cases;
On the right side in 20 cases.

Emphysema of the upper left lobe was found in 9 cases.
" of the upper right lobe was found in 12 cases.
" of the lower left lobe was found in 15 cases.
" of the lower right lobe was found in 2 cases.[1]

In the remaining case the emphysema was limited, on the right side, to the middle lobe and the contiguous parts of the upper and lower lobes. And as the facts above detailed are sufficiently numerous, we may from thence conclude that very probably both lungs are equally subject to the disease. The extent to which it is carried, too, appears to be the same in each, for although it was most developed on the right side in four cases where the dilatation was general, the same was true of the left in an equal number, and in the remainder the size of the pulmonary vesicles was apparently the same on both sides.

Further, whatever was the extent of the dilatation, it was never uniform, the cells being more dilated in one part than another, and especially upon the internal surface and at the base of the lungs. When the vesicles, thus inordinately dilated, happened to be very numerous in a given point, they formed, by their reunion, flattened prominences of various forms and dimensions. It is also worthy of remark that in every case where the relative side of the vesicles was noted with great accuracy, (fifteen,) they were found more voluminous on the free border than elsewhere. This fact is important, and seems to show that emphysema is independent of pulmonary catarrh, or at least of that form of it which has its seat in the smaller ramifications of the bronchial tubes, and is marked by

[1] There is clearly an error in this table, which does not accord with the previous ones. It should stand as follows:—

Upper left lobe 9.
Upper right lobe 10.
Lower left lobe 1.
Lower right lobe 4.—*Translator.*

a subcrepitant râle, inasmuch as this form of catarrh, as we shall see hereafter, does not occupy the anterior portion of the lungs.

The free border of the lungs is so emphatically the part in which the cells have the greatest tendency to dilatation, that when the latter has arrived at its maximum, or when the vesicles have been ruptured, and thus have formed those peculiar appendices mentioned in the general description, it is still along the free border that this occurs, either anteriorly or at the base of the organ. These appendices were found in three cases, of which one only belonged to those attacked with cholera. Hence, in whatever way we view the two series of observations before us, we uniformly find that the extent and degree of the emphysema were always in direct proportion to the age of the patients, or to what in an extremely chronic complaint is the same thing, the duration of the disease.

Another remarkable fact, which confirms the result of the previous analysis, and shows that both lungs are equally disposed to become emphysematous, is that in the three cases just mentioned appendices were found on both the right and left sides.

The following case, although deficient in many respects in the history of the symptoms, is nevertheless sufficiently interesting in an anatomical point of view to be introduced here.

CASE I.

Cough for the last six years; oppression during five, aggravated during the last few, months—Pain in the larynx, and, at the same time, aphony—Epileptic paroxysms—Cancer of the larynx, general emphysema, with appendices—Dilatation and hypertrophy of the heart.

A man, aged sixty-eight years, at one time a workman in saltpetre, but who, for the last fifteen years, had been employed as a cook, was admitted into the hospital of La Charité, St. John's ward, the 18th of November, 1822. Of middle stature (five feet two inches), of a lively disposition, and generally in the enjoyment of good health, he had been attacked with an acute affection of the chest at the age of fourteen years, had been subject to cough for the last six, and to difficulty of breathing for the last four. The dyspnœa had become considerable five months since, and on this account the patient was obliged to enter the hospital for the first time, where he remained three months and a half. At that time, his voice was altered in character without being entirely lost; he was affected, at intervals, with palpitation of the heart, and complained of headache, to which he had been subject for many years, and which had very much increased during the last six; frequently, also, these headaches had been accompanied with dizziness, without loss of consciousness. At no time had there ever been pain in the throat, or the sides of the chest, or hæmoptysis. On the present, as on the previous occasion, the patient came to the hospital on foot, and was taken, immediately on his arrival, with loss of consciousness, trembling, agitation, accompanied with slight foaming at the mouth.

The following day, the 19th, emaciation very great, lips slightly of a violet hue, skin more or less tawny, or of an earthy tinge throughout; has no recollection of the spasmodic attack of the evening before; cheerful; no headache, no pain in the limbs, recollection accurate, lies with his head a little elevated, as has been his custom from childhood; respiration slow, a slight feeling of oppression in the middle portion of the chest, aphony, cough not frequent but very hard; a few expectorated masses of a grayish colour, and demi-transparent, or else greenish and opaque; percussion very sonorous throughout the whole extent of the chest; inspiration whistling, or rather a whistling noise during the expansion of the chest, without any respiratory sound except here and there when bronchial respiration is heard. Beating of the heart, accompanied with impulsion at the præcordial region and irregular, so that at every fifth beat there is one wanting; pulse scarcely accelerated, anorexia, no thirst; tongue whitish, furred; an obscure redness, slightly marked, upon the anterior half arch of the soft palate, on the right side; deglutition easy; belly flat, soft, not painful on pressure; two liquid stools as usual for last five or six weeks. (Pect. gum pot., with fifteen drops of the ethereal tincture of digitalis.)

From this time to the 12th of March, the day of his death, during a period of nearly four months, I made the following observations:

Auscultation and percussion were practised nineteen times, and always with the same results, except that sometimes the respiratory sound was entirely absent throughout the front of the chest; both the sibilant and sonorous râles varied in intensity at different times; and there was no paroxysm of dyspnœa.

The beating of the heart continued the same, both as regards the impulsion and the extent to which the sounds were heard; but their irregularity was so far from being permanent, that after the 22d of November they no longer presented that character; from this time the pulse was uniformly calm and regular, with only two exceptions of two days' duration, on the 14th of January and 11th of March. The loss of voice was permanent, and the cough continued hard (déchirée); the expectoration varied only in quantity, and it seemed to the patient as though it was detached from the neck, at a part which corresponded to the upper extremity of the sternum.

The appetite soon returned, so that by the middle of December the patient ate a demi-portion; this quantity, however, was afterwards diminished, in consequence of the spasmodic symptoms, which made their appearance on the 14th of January; notwithstanding this, however, the patient ate, on the 22d of the same month, a quarter; on the 25th, a half; and on the 28th, three quarters of a portion. Low diet was again resorted to, on account of a diarrhœa which supervened, and from this time to the day of his death, the patient took only soups. No difficulty of deglutition was apparent, except upon the 16th and 17th of January, when the patient returned a little of his drink by the nose. Nausea occurred

only once, and that was on the 3d of January, when the kermes mineral had been given in doses which had been successively increased to eighteen grains in the twenty-four hours. Previous to the 10th of December, there were three liquid stools a day, after which they were less frequent, or one a day, notwithstanding the administration of the kermes. On the 3d of February, the diarrhœa returned with greater severity than before, and so continued until the 15th, during which time the number of stools varied from six to ten a day, and afterwards from one to three.

The delirium, which had disappeared at the time of the first visit, returned upon the evening of the same day, the 19th of November, and was not completely dissipated until the 22d. On the 14th of January, at ten o'clock in the morning, sudden loss of consciousness, universal spasmodic movements extending to the muscles of the face, foaming at the mouth for two or three minutes, followed by a cadaverous paleness of the countenance, with enlargement of the right pupil, and considerable prolongation of its vertical diameter. Four minutes afterward this disorder disappeared, without the consciousness being restored. On the following day, the intellect was merely more dull than usual; on the 16th, there was delirium during the whole day, great restlessness during the two following nights, so that the patient had to be kept in bed by force, and his mind was not perfectly restored until the 21st. The delirium again returned on the 7th of March; on the 11th, about half an hour after the visit, return of the same symptoms as on the 14th of January; the prolongation of the right pupil having occurred a second time on the 17th of January, merely for a few hours.

The patient was bled from the right arm on the day following his admission into the hospital, on account of the cerebral symptoms; this operation was repeated the 16th of February, as well on account of a little tenderness at the epigastric region, as in consequence of the beating of the heart, which was heard over the whole chest. From the 16th of December to the 2d of January, the kermes was given in doses gradually increased from two to eighteen grains; afterwards it was discontinued, on account of nausea, and the oximel was employed instead, towards the end of the month, during ten days, after which it also was discontinued, on account of the diarrhœa.

Examination of the body thirty-seven hours after death.—*External condition.*—No oozing from the body; no infiltration; discolorations only on the posterior part of the body.

Head.—Numerous drops of blood upon the exterior surface of the dura mater; moderate infiltration under the arachnoid; an ounce of serum in each lateral ventricle, less limpid in the right than left; brain moist, but little injected; nothing else worthy of note.

Neck.—The epiglottis was healthy, but turned a little to the left. Immediately beneath it was found a white substance, hard, firm, difficult to cut, creaking under the scalpel, shining, without any distinct structure, extending backwards, and to the right side,

between the thyroid and cricoid cartilages, not extending superiorly above the level of the thyroid, and but a little behind the arytenoid cartilage of the right side, which was transformed into a substance resembling that above described, without, at the same time, being completely confounded with it. The cancerous mass above mentioned pushed the epiglottis a little to the left, was three quarters of an inch thick behind, a little less in front, and, consequently, was shaped like a wedge, the larger extremity of which looked toward the vertebral column. Softened, and of the consistence of cream, internally, it diminished very much the cavity of the larynx on the right side, where the vocal cords were destroyed; and this contraction was still further augmented by the development of another cancerous tumour, similar to the first but much smaller, and situated under the left inferior vocal cord, which was bent inwards. This latter, together with the upper one of the same side, was free from ulceration. The thyroid cartilage was healthy, the cricoid was ossified throughout its whole extent, and the arytenoid muscle of the right side terminated insensibly in the cancerous mass which occupied its place. The mucous membrane of the trachea was pale, and of the usual consistence and thickness throughout.

Chest.—Not a particle of serum in the cavities of the pleura; some loose cellular adhesion between them and the lungs. The latter did not completely fill the cavity of the chest; were of a reddish colour posteriorly, whilst in front they were whitish or grayish. The left one presented over its whole surface a remarkable dilatation of its cells, the size of which varied from that of a millet seed up to that of a cherry stone. These latter formed prominences, which, when opened, presented little round cavities, without any false membrane. These were rarely found, except upon the free border where the emphysema was much the most marked. Besides these, were found, along this free border, both superiorly and inferiorly, and at the base, three sorts of appendices of an elongated form, thin at their extremities, resembling, both as regards their shape and lightness, the air-bladder of fishes; emptying themselves completely when slightly pricked, like a leather bottle, and presenting internally several bands of cellular tissue, irregular and of a honeycomb appearance at their circumference. The vesicles of the right lung were not less dilated than those of the left, and, as in the latter, appendices were also found along its free border. These appendices were two in number: the one was as large as a medium sized nut, and did not entirely empty itself till after it had been twice pricked at its extremities; the other was larger, being nearly four inches long by one and a half broad, and in shape like a kidney. This last one was dried and then divided transversely at the distance of about an inch from each end, by which means its structure was found to be as follows:—One of the cut surfaces presented, at its centre, some vesicles smaller than a grain of millet seed, circumscribed by transparent partitions, and very thin. These cavities, or cells, became larger in proportion as you approached the circumference, near which their size was

equal to that of a grain of hemp seed, or even larger; they were of an irregular form, oblong, and, as it were, toothed. The other portion of the appendix was a little larger, and presented three principal vacuities of the size of a small nut, and traversed by cellular bands, and also cells, separated by very thin walls, shining like the coats of an onion, from three to four lines long by about as much in breadth, and traversed by several vessels which were not easily recognised by their colour. The posterior part of both lungs contained a considerable quantity of reddish serum, and at their summit was a small portion of demi-transparent grayish matter. The bronchial tubes were rather contracted than dilated. The pericardium was healthy, and the heart larger by half than natural. The walls of the left ventricle were seven lines thick about the middle, and those of the right from three to four at the point, and for an inch beyond it, but not more than half that thickness elsewhere. The free border of the tricuspid valve was opaque and a little thickened, without much hardness; that of the mitral valve more or less cartilaginous, and above it were found some yellowish plaques. The semilunar valves of the aorta were more or less cartilaginous at their base, and two of them had become converted into one, separated inferiorly by a thick septum. The aorta, at its origin, was of a dull yellow colour, and presented, throughout its whole extent, a great number of white, shining, semi-cartilaginous plaques, which studded the orifices of all the intercostal arteries. Besides these, there were also some yellowish plaques, three of which were ulcerated, blackish, and of a pulpy consistence upon the surface. The lesions above mentioned extended into the iliac arteries, as well as the popliteal and femoral, and especially the latter, where they were more advanced than in the femoral.

Abdomen.—Œsophagus healthy; stomach a little larger than natural. The entire surface of its mucous coat was covered with a pretty viscid mucus, mamelonated, of a slate colour interspersed with reddish spots occasioned by finely injected vessels, thinner than natural, but of good consistence, except in the great cul-de-sac, where it was a little softened. The duodenum was healthy. The small intestine contained a little bile in its upper sixth, and below that mucus; its mucous membrane was perfectly normal. That of the colon was in contact with pultaceous fæcal matters, very much softened, and studded throughout its whole length with numerous crypts of the size of millet seed, except in the rectum, where they were a little larger. The liver and gall-bladder offered nothing worthy of note. The same was true of the bladder and kidneys. The spleen was scarcely a fourth of its usual size—its tissue firm and pale.

I shall not stop to consider all the various symptoms experienced by this patient, or the cancer of the larynx which certainly hastened his death; neither shall I speak of the heart, and especially of its right ventricle, the walls of which were so remarkable on account of the circumscribed hypertrophy of which they were the seats. I go on to observe that, contrary to what usually takes place in

analogous cases, the lungs did not entirely fill up the cavity of the chest. Notwithstanding this, the vesicles were exceedingly dilated; and in accordance with what was found in all the other cases in which this point was sufficiently attended to and noted, the greatest degree of dilatation existed near the free border of both lungs, and at the base of one of them, so that the appendices or excessive emphysema were found only in these parts. These appendices were remarkable in this—that when the cells did not appear to be evidently ruptured, the latter became larger as you approached the circumference: a circumstance which perhaps is true also of the lungs, but which it would be well to verify. I would also call attention to the fact, that, wherever the cells in the appendices were freely ruptured, the septa dividing them were also very thin, as if, contrary to what usually takes place, the walls of the cells in expanding had not become thicker. Unfortunately, I neglected to make use of the easiest means of determining this fact—that is, of pressing in different parts the tissue of the lungs, and determining whether, after being thus deprived of air, their parenchyma was thicker than natural, or offered any appreciable alteration in this respect. Hence, although the supposition that the walls of the vesicles were not thickened is exceedingly plausible, it cannot be regarded as strictly demonstrated. It is very plausible, because through its means we can account for the collapse of the lungs, and can also, perhaps, explain in a satisfactory manner, in accordance with views which I have already expressed, how, notwithstanding the lungs were so extensively diseased, the dyspnœa was so trifling.

The structure of the appendices as above detailed was found also in the two other cases in which they existed. Their lightness was remarkable in every case, and they emptied themselves almost entirely after a single prick—displaying internally a large cavity traversed by numerous filaments, whilst at their circumference existed numerous supple flattened vessels, probably venous, the diameter of some of which was half a line.

As the pulmonary vesicles are nothing more than the termination of the bronchial tubes, we should hence be led to conclude that both would be similarly affected; that when the vesicles were dilated, the bronchial tubes would also be so to a certain extent. This, however, is not the fact, as indeed Laennec has already stated; for, of thirteen cases in which the bronchial ramifications were examined with care throughout their whole extent, four only were examples of the dilatation of which we are speaking, and that confined to a few branches—a proportion which probably is not greater than would be found under other circumstances in persons of the same age as those of whom we are speaking.

Besides, we cannot suppose that in the above four cases there was any connection between the dilatation of the cells and that of the bronchial tubes, as the latter was found to be exceedingly limited in extent, whereas the former was general. It would further seem that mere chance had nothing to do with the above result;

for in two of the cases where the lungs presented appendices, and where the bronchial tubes were described with care, these last were not at all dilated, but appeared, on the contrary, to be rather smaller than usual in one of the cases (Case 2). I will just add, that where both lungs were emphysematous the dilatation of the bronchia was found only on one side, at least in the great majority of cases; and further, that although the vesicles were more enlarged near the free border of the lungs than elsewhere, the dilatation of the bronchial tubes was not particularly marked in that part. One of the succeeding observations affords a striking example of this fact.

Adhesions between the lungs and pleura were found in the great majority of the cases—in thirty out of thirty-six; and perhaps it would be natural to infer that the emphysema exerted some influence in the production of this lesion. But, in the first place, we must inform the reader that universal adhesions of both lungs did not exist in the same individual in a single case; that general adhesions of one side were found in only fifteen; that in the others they were limited to one of the lobes, and oftentimes to an extremely small portion of their surface: so that, in this respect, the facts before us are not very different from what we observe under other circumstances in persons not tuberculous. It should also be observed that the individuals, whose history we are analysing, were generally more advanced in life than those who die of most other diseases—which circumstance removes the difference above mentioned, or rather explains it, since the general causes of pleurisy had operated for a shorter time in the one case than in the other. But what renders it perfectly clear that the adhesions of the pleuræ were not dependent upon the emphysema, is, that the lungs were perfectly free throughout, in two out of the three cases in which the emphysema was greatest; and that, as usual, the adhesions, when they were not universal, were found on the posterior part, whilst the greatest degree of dilatation of the vesicles was found on the anterior part of the lungs.

Thus, on the one hand, the adhesions of the pleuræ, or rather the inflammation which gave rise to them, was governed by laws different from those of emphysema; whilst, on the other, the bronchial tubes were commonly healthy, and their dilatation, partial when it did exist, was probably not more frequent in the cases of emphysema than among those who die of any other disease. Hence it follows that, whatever may be the cause of the dilatation of the vesicles, we cannot regard as such either an inflammation of the surrounding parts or of the organs of which they are a continuation; neither can we admit that this dilatation becomes a cause of inflammation in the neighbouring textures. Thus, we see that every circumstance leads us to conclude that inflammation has nothing to do with the development of that form of emphysema which we are investigating, and that the thickening of the parietes of the vesicles, which might occasion some doubts in this respect, was in fact a simple hypertrophy, occasioned, as in most other similar cases, by mere distension.

There is still another lesion which deserves to be noted amongst those appertaining to the lungs—I mean tubercles. These, or gray demi-transparent granulations, were found in ten cases, and always in small number, except in one instance, where the gray demi-transparent granulations were pretty generally distributed. In fact, tubercles were rather less common in persons affected with emphysema than in those who died of other diseases, and whose history I have preserved; consequently, it is impossible to attribute their production in the least to the emphysema. Another proof of the correctness of this conclusion is found in the fact that the occurrence of tubercles was not more frequent in those cases where the emphysema was most advanced, than in those placed in opposite circumstances—for they were found six times among the latter, and only four in the former; or, in other words, they were rather more frequent among those who were carried off by the cholera, and in whom the emphysema was less advanced, than in the others. It would be easy to account for this difference, if the smallness of the number did not induce us to regard it as a mere coincidence, by recollecting that those who died of the cholera were younger than the others, and that the frequency of tubercles is always in an inverse ratio to the age. Nevertheless, as was mentioned before, the patients whose histories we are analysing had been subject to frequent attacks of pulmonary catarrh of various degrees of severity, and many of them had not been free from it for a number of years; so that this small series of cases, as far as it goes, shows us how wide is the difference between pulmonary catarrh and tubercles, as well as that the latter is totally independent of the former.

§ II. *Of the Heart.*—The condition of the heart deserves to be studied especially on account of the functional relations which exist between it and the lungs. The heart was larger than natural in sixteen cases, being double its usual size in one case, and rather more than double in two others. But these cases were not equally distributed among the two series of cases whose history we are examining. Six of them only belonged to the twenty-three cholera patients, whilst the remaining ten formed part of the nineteen individuals carried off by other diseases. The latter, as the reader will no doubt recollect, were more advanced in life than the former; and he will think perhaps that the difference above mentioned is owing to this cause, especially as the mean age of that portion of the cholera patients in whom the heart was found enlarged, was the same as that of the corresponding cases in the other series. But this view of the subject will be abandoned, and it will, on the contrary, be admitted that the emphysema itself had something to do with the production of this large number of cases of dilatation of the heart, as soon as I mention that the amount of enlargement of this organ was much the least among the cholera patients, in whom the emphysema was generally more marked than in the others.

ARTICLE SECOND.

EXAMINATION OF THE SYMPTOMS.

I shall begin with an account of the dyspnœa, after which I shall investigate successively the configuration of the chest, the results of auscultation and percussion, the cough and expectoration, the derangements of the circulation, and the condition of the digestive and nutritive functions. I shall then pass to the study of the diagnosis, causes, progress, and frequency of the disease; and, lastly, to its treatment. I shall examine the symptoms separately in those patients who died, and in those who left the hospital more or less relieved, commencing with the latter, whose history is most complete.

§ I. *Of the Dyspnœa.*—1. *In the patients who quitted the hospital more or less relieved.*—Dyspnœa occurred in forty-two out of forty-four cases where it was carefully studied. The two patients who thus present such a remarkable exception were aged, the one twenty-one years (a woman), and the other thirty (a man), and were not habitually subject to pulmonary catarrh. It is impossible that any doubt can exist in reference to the above statement, either as regards the absence of oppression, considering the care with which the patients were interrogated, or the correctness of the diagnosis, since both the individuals alluded to presented the symptoms of emphysema to a remarkable degree, and were repeatedly and carefully examined. Hence we must admit that pulmonary emphysema may exist without dyspnœa, at least for a time, and at a period probably not very remote from the commencement of the disease, which, however, it is impossible to determine certainly, unless the patients could have been observed from their infancy. Do the above cases belong to that class in which the dilatation of the vesicles takes place, or appears to take place, without hypertrophy ?[1]

The oppression, after having once shown itself, continued without interruption in every case, except in a young man of eighteen years of age, who was not subject to catarrh, and who entered the hospital on account of a paroxysm of dyspnœa, of moderate severity, which disappeared in eight days. This was the second attack which this young man, who was very intelligent, had experienced. The first happened two years before, and lasted the same length of time, eight days; and in the interval the respiration had been perfectly free, whether the patient worked much or little, walked rapidly or slowly. In this case, too, as in those who never had dyspnœa at all, all the physical signs of emphysema were present, and were observed both during and after the paroxysm of dyspnœa. The period at which this symptom showed itself was very various. Its first appearance

[1] Quite lately I met with a well-marked case of emphysema in a woman of thirty years of age, who has never had dyspnœa. I interrogated her on three successive days in reference to this point, and each time she assured me that no one could go up six flights of steps more rapidly, and experience less difficulty of breathing.

was traced to infancy in sixteen patients, who recollected that they could never run as rapidly as other children of the same age, at least without being quickly put out of breath. In the twenty-six who remain, the dyspnœa commenced later in life; before twenty years of age in two cases, from twenty to thirty in seven, from thirty to forty in the same number, from forty to fifty in eight, from fifty to sixty in three. These facts are remarkable, and indicate that after the age of fifty years the occurrence of emphysema is scarcely to be feared, although, as previously stated, the lesion continues to increase after that period.

The oppression, commonly slight at its commencement, augmented after a lapse of time which varied very much in the different cases. In those in whom it showed itself during infancy, it was for the most part very trifling for a great length of time, so that of eleven patients of this class, it increased during childhood in only three, after the age of fifteen in another, and from twenty-two to fifty-eight in the remainder. In other words, the oppression, except in the three cases above mentioned, did not increase until about the same period at which it commenced in the others. This fact accounts, to a certain extent, for the advanced age of several of our patients, some of whom appeared to be very far from the termination of their career.

Of eighteen patients in whom the dyspnœa commenced later in life, and where it was carefully studied, twelve experienced it to a moderate degree and uniformly, for a period which varied from two to twenty-six years, or during a mean term of ten years. In the six others, the oppression went on gradually increasing from its commencement, and in two of them it was from the first pretty severe. The *paroxysms* of dyspnœa made their appearance, in the majority of cases, simultaneously with the augmentation of the oppression; this coincidence occurring in thirteen out of the seventeen patients in question. The paroxysms appeared later in the others, and were mostly slight with those who had been subject to oppressed breathing from their infancy, besides which they did not occur in more than half of the latter, or six out of thirteen.

Are the paroxysms in question really less frequent and severe in individuals who have been short breathed from their infancy, than with those who have experienced it for the first time later in life? or is it merely a coincidence? Time and observation will decide.

These paroxysms, the severity of which was sometimes extreme, frequently supervened as before stated, without any appreciable cause, but most commonly in consequence of an attack of acute pulmonary catarrh. In one case, for an account of which I am indebted to Mr. Boudier, they occurred only at night, at which time the patient was obliged to sit up, or even go to the window to breathe. I am unable to say whether atmospheric changes exert an influence upon their production, having neglected to obtain accurate information upon this point, which is in fact rather difficult to come at. But I am sure that if this influence is real, it is at any rate not constant. Of this fact I lately met with a convincing

proof in a woman sixty years of age, with whom the first symptoms of emphysema dated five years back, and whose dyspnœa both augmented and diminished, under the same atmospheric influences, during a residence of six weeks in the hospital.

This dyspnœa, so remarkable on account of the early period of life at which it so frequently commenced, of its duration, its permanency, its frequent exacerbations, unaccompanied, for the most part, by any other sign of disease of the heart; this dyspnœa, I repeat, was of itself very characteristic of emphysema. For it was impossible to attribute it to a simple habitual pulmonary catarrh, which was not present in every case where the oppression dated from about the twentieth year, or later, and was entirely absent in those where it dated from infancy, except in a single instance. It was equally impossible to refer it to a partial or general dilatation of the bronchial tubes, because the dyspnœa which sometimes accompanies the latter is of moderate severity, and especially is not paroxysmal. In some instances where there was complication of disease of the heart, the latter was only of a few years standing when the patients came under our observation, and did not by any means date from early life. Finally, the absence of hæmoptysis, and of the other symptoms of tuberculous disease, precluded altogether the supposition of phthisis. From all this it is evident, that dyspnœa, *such as it has been described*, is of itself sufficient to indicate, as it has in fact frequently indicated to us, the existence of emphysema of the lungs.

The following fact, for which we are indebted to J. Jackson, is a fresh proof of the value of dyspnœa as a diagnostic symptom in the affection before us. Out of one hundred and twenty individuals of whom he enquired into the state of the respiration from infancy, he found twenty-eight who had been subject to more or less shortness of breath since that time. Of the latter, one was affected with disease of the heart, two with phthisis, and the remainder with pulmonary emphysema. Hence it was concluded by the same physician that dyspnœa, the origin of which may be traced to infancy, and especially if it is paroxysmal, is a symptom almost pathognomic of emphysema. The following case, recorded by Jackson, shows that dyspnœa, even of no very great severity, is still highly important in the diagnosis of emphysema.

CASE II.

Dyspnœa from infancy, frequent palpitations dating from the same period; thorax generally more rounded than common, partial prominence on the right side anteriorly —Lungs not collapsed, coming in contact with one another at their edges; pulmonary vesicles dilated in the upper lobes and throughout the middle lobe of the right side, &c. &c.

Blanchon, weaver, aged 58 years, four feet five inches in height, hair grayish black, eyes gray, of a moderately strong constitution, was admitted into the hospital of La Pitié the 17th of June, 1833. Having been engaged in military service from his twenty-seventh to his thirty-sixth year, he had been attacked, when twenty-eight

years old, with a severe pneumonia, (cough, bloody expectoration, oppression, fever, delirium,) and afterwards, at the age of thirty-four, with a tertian fever. Subject to short breath from infancy, his dyspnœa had never much increased, even after the attack of pneumonia above mentioned. Since his twentieth year, he had been liable to frequent attacks of cold in the head, which lasted during the first few years from four to five days, and afterwards from ten to twenty, but never obliged him to suspend his daily avocations. From infancy, also, Blanchon has been subject to palpitations which increased after the attack of pneumonia, but were never very inconvenient, or accompanied by œdema of the lower extremities. Patient states that he was taken sick four months ago, was not obliged, however, to keep his bed, but had always been able to accomplish half a day's work.

At first, dry cough, augmentation of dyspnœa, chilliness; afterwards, persistence of the same symptoms with heat and sweats during the night; expectoration came on about the middle of May, and since March there has been diminution of appetite and progressive emaciation: no hæmoptysis, no pain in any part of the chest.

June 20th. Head elevated, expression of uneasiness and anxiety, lips a little livid, speech interrupted, memory sluggish, emaciation considerable, very little appetite, thirst great, tongue whitish and moderately moist; pain in the throat from coughing, digestion easy, stools not frequent, flying pain in the abdomen; tension and pain on pressure under the right false ribs, without any alteration in the form of the belly; cough very frequent, severe, and painful; expectoration greenish, opaque, not aerated, demi-liquid, or thicker; oppression great, respiration forty; thorax a little more convex anteriorly and posteriorly than common, right supra and infra-clavicular regions a little more prominent than the corresponding ones at left; on the right side, also, there was a slight partial prominence extending from the clavicle to the third rib along side the sternum; intercostal spaces ill defined on both sides; percussion every where sonorous, much more so behind on both sides, in the lower third of the chest, than elsewhere, and also a little more so under the left clavicle, for a distance of about two inches, than on the opposite side; sonoriety a little less in the præcordial region than in the corresponding point of the right side; respiration everywhere feeble, (except at the right summit,) more so at left than right, especially behind and laterally, a little less feeble at the left posterior and inferior third than at the summit of the same side. Immediately under the right clavicle and at about the same level posteriorly, the respiration was strong and very prolonged, with bronchophony at the latter point. Sonorous râle anteriorly throughout, subcrepitant râle in the right lower third behind. Pulse regular, not large, 72 in the minute: pulsations of the heart a little dull, heard throughout, with impulsion. Heat moderate, although the patient thinks it is very great; slight sweat at night; cephalalgia. v. s. 3xii. on the 19th had produced a little relief.

21st. Subcrepitant râle on both sides behind, and even a little

anteriorly on the right side at the base of the chest; (one grain of opium in two doses).

23d. Evening. Pulse 90. The right arm, in which the bleeding had been performed, red, tense, swelled at the bend of the elbow, and painful from the armpit to the fingers; heat universal; (emollient cataplasms).

In the evening of the 25th, chills; 26th, whole right arm very much swollen; 27th, delirium, tongue brown, dry, and thick, pulse 120: the subcrepitant râle continued.

The same symptoms still persisted, accompanied by drowsiness, and the patient died on the 30th, at three o'clock in the morning.

His father had died at the age of 90 years, unaffected either with asthma or paralysis. His mother had deceased at the age of 22, and his brothers quite in early life; but he is not able to say what was the nature of the disease by which they were carried off.

Opening of the body thirty hours after death. Exterior condition.—Skeleton well formed; some discolourations on the abdomen and limbs; right arm a little larger than the left, at the elbow and near it, without œdema or redness; orifice where venesection was performed gaping; skin slightly thickened at the bend of the elbow for the length of three inches; the corresponding subcutaneous cellular tissue, yellowish, thickened, not elastic, very adherent to the veins and brachial aponeurosis; median basilic and cephalic veins firm, flattened, and double the usual size; same condition of the basilic and cephalic veins at their origin, for an extent of from two to four inches, from the radial and cubital veins near the bend of the elbow. The basilic vein contains no liquid; its walls are half a line in thickness, pale and polished, even in the neighbourhood of the axillary vein, where they are lined for an inch in length with a false membrane, of a rose colour, irregular, but slightly consistent, easily raised up, and which does not obliterate the cavity of the vessel. The median basilic is not less pale, and presents also internally a false membrane, which is only a few lines in length. The radial and cephalic veins are obliterated, near the bend of the elbow, for about an inch, by a reddish clot of tolerable firmness. The veins which are continuous with those of the arm are, like those of the left side, in a natural condition. The same is true of the arteries and nerves throughout the whole limb.

The articulations of the knee contained each about two tablespoonfuls of a purulent liquid, mixed with white flocculi; their surfaces are pale, except a slight injection in the neighbourhood of the adipose ligament. The other articulations are perfectly healthy.

Head.—A few glands of Pachioni; no infiltration under the arachnoid; pia mater very slightly injected; a small spoonful of clear fluid of the colour of wine lees, in each of the lateral ventricles. Nothing remarkable in the interior of the cranium.

Neck.—Larynx and trachea pale, without any appreciable changes from the natural condition.

Chest.—Lungs not collapsed, approaching one another at their

borders, but not absolutely in contact; adhesion at the summits much greater on the right side than on the left. In the same point, also, they are furrowed with wrinkles of from six to eight lines in length, deeper at right than left, across which pass, in the form of a bridge, small and thin cellular bands, which are likewise strongest on the right side. On the left side the upper lobe presents vesicles, varying in size from that of a grain of *semouille* to that of a small pin's head, both on its internal and external surface, over a third part of its extent, in the neighbourhood of the free border, which was rounded. Elsewhere the volume of the vesicles was not appreciable, so small were they. On the right side in the inferior third of the superior lobe, and only at its anterior portion, their dilatation was about the same as on the opposite side. The dilatation existed to about the same degree in the whole middle lobe, and for the length of two inches in the contiguous part of the lower lobe, extending laterally about as far as on the opposite side. Elsewhere the vesicles were scarcely or not at all apparent. The left lung was slightly infiltrated behind and inferiorly with a red fluid, whilst at the summit its tissue was indurated for a line and a half in depth. In the same part of the right lung there was found a cavity of the size of a nut, containing a small quantity of demi-fluid matter of a dirty white colour, and lined by a false membrane, which was irregular, soft, and easily detached from another one which was more polished and firm, and connected with the surrounding cellular tissue, which was dense for a line and a half beyond. The inferior lobe was like the corresponding one on the left side. There were no tubercles or gray demi-transparent granulations in either lung. Two bronchial tubes, which were neither red nor thickened, opened into the cavity of the right lung, and were continuous with a larger trunk, the mucous membrane of which was thickened. Nothing similar on the left side. The pericardium contained a spoonful of reddish serum; the heart was flabby, of the usual size, and contained coagulated blood. The internal membrane of the ventricles was of a red colour. The walls of the left ventricle were five lines thick, and those of the right a line and a half at the middle portion.

Abdomen.—Stomach of moderate volume, containing a small quantity of yellow fluid. Mucous membrane pale, mamelonated anteriorly in the pyloric half, and for two inches in breadth posteriorly, where it gave strips only two or three lines long. Small intestine moderately distended with gas, and by a small quantity of yellow or whitish liquid. Its mucous membrane was perfectly healthy, except in the middle third, where strips of only two or three lines could be raised. In the two last thirds of the large intestine were found hard fæcal matters. Its mucous membrane also was perfectly healthy, except in its first half, where strips of only four to six lines long were obtained. The liver was healthy, and the gall-bladder filled with a deep green fluid. The spleen was double its usual size, of the colour of wine lees, and easily penetrated. Pancreas healthy. The right kidney swam in water,

on account of the development of a considerable quantity of air beneath its proper coat. Its tissue was softer than that of the left side. The cortical substance of both was of a livid red colour for the depth of a line. Bladder quite healthy.

In the above case, we see that the dyspnœa, which was traced to very early life, had never been very considerable, and that the dilatation of the pulmonary vesicles was likewise not excessive, or, in other words, that there was a perfect correspondence between the one and the other. The oppression cannot be referred to dilatation or hypertrophy of the heart, no traces of which were to be found; or to habitual pulmonary catarrh, or dilatation of the bronchial tubes, which did not exist; or even to the tuberculous disease, the commencement of which was still recent at the time of the patient's death. By this method of exclusion, then, we are forced to account for the dyspnœa of Blanchon by means of the dilatation of the vesicles; and as this symptom was present from early life, we are forced, also, to date the commencement of the emphysema from the same period.

It is worthy of remark, that the oppression did not sensibly increase after an attack of pneumonia, which the patient experienced; for it shows that we cannot with certainty attribute emphysema to pneumonia, when the former makes its appearance soon after an attack of the latter. This caution is the more necessary, as it is not a very uncommon thing to meet with persons who have had two, three, four, six, or even a greater number of attacks of pneumonia, but who are, at the same time, free from shortness of breath, or any other symptom of pulmonary emphysema. Nevertheless, at first sight, and considering merely the anatomy of the parts, one would be disposed to form an entirely different opinion. In fact, when a lung, which is in the second stage of inflammation, is cut into, it presents an appearance of hepatisation, which is evidently owing to the accumulation of a more or less thick or concrete matter in the pulmonary vesicles. Under these circumstances the diameter of the latter is greater than usual, and one would imagine that this dilatation, which occurs in almost every case of pneumonia, (for it is almost always accompanied by more or less hepatisation,) would persist after the termination of the disease. But it is not so; for when the affection is dissipated, the cells resume their ordinary dimensions. Hence it is clear, from this as from a thousand other proofs, that nothing can supply the place of direct observations.

As to the correspondence which we have said existed between the symptoms and the lesions, it should be observed that, although the emphysema appeared to exist to about an equal extent on each side of the chest, the respiration was more feeble on the left than on the right; and that, on the contrary, there was on the latter a prominence which did not exist upon the opposite side. We may explain, to a certain extent, the greater strength of the respiratory murmur at the upper part of the right lung, by means of the excavation which was found at its summit, which, as it gave rise to

bronchial respiration, must also have altered the vesicular murmur. But how are we to account for the prominence, unless we suppose it to have been congenital, or admit an error of observation? The last would not appear very probable to any one who knew the gentleman who recorded the above case, and the time and attention which he devoted to the determination of facts.

Further, although emphysema was not proved to exist posteriorly on either side, it is yet very probable that the vesicles were dilated there; for the convexity of the chest, its extreme sonorousness, and the feebleness of respiration in that region, put the fact almost beyond question, at the same time that the engorgement would not allow of its being positively demonstrated.

It is scarcely necessary to observe, that there was a perfect accordance between the alterations of the venous system and the symptoms observed after the bleeding, and that death was owing, not to the emphysema, but to an accidental lesion.

2. *In the fatal cases*, the oppression of breathing was less minutely investigated, and, consequently, its history is less perfect than in the previous series. The following is a summary, however, of what was observed.

Difficulty of breathing was found in every case, but varying both in intensity and duration. In seven of the eighteen cases now before us, it existed from infancy. It commenced later in life in the others; before the twentieth year, at the ages of ten and eighteen, in three individuals, aged twenty-nine and fifty-five years; at twenty and thirty years old, in two cases; at from thirty-five to seventy-one, in the remainder.

In two out of thirteen cases where this matter was carefully examined, the amount of dyspnœa was always the same, or at least did not sensibly augment. One of those in whom it dated from the very earliest infancy, and whose history has just been given, was of this number. In the others, the dyspnœa augmented after the ages of thirty, forty, or fifty years, but commonly long before the period at which, in some instances, the first symptoms of disease of the heart made their appearance. It was not severe at the outset, except in one individual, a man of seventy-four years of age, and who died of cancer of the stomach.

Paroxysms of dyspnœa are noted as having been present in three cases, in two of whom difficulty of breathing had existed from early infancy. In a fourth case, it is mentioned that they did not occur at all, at least during the time that the patient was under observation (case 1).

§ II. *Configuration of the Chest.*—1. *In the patients who quitted the hospital more or less relieved*, the form of the chest was carefully examined in thirty-seven cases, and in every one it had undergone some change from the natural condition. This change was uniform throughout the whole chest in a young woman twenty-three years old, whose dyspnœa dated from early infancy, and had become paroxysmal about three months before her admission into the hospital. Her chest was every where more prominent

than in the natural condition, approaching to a globular form, and equally so in corresponding points. This prominence was formed both by the ribs and the intercostal spaces, which latter were not depressed as they are in the natural condition, especially in thin persons. In the other cases, the change of form was similar in character, but partial in extent, the prominence being limited to one side of the chest, except in three cases, where two prominences were observed, whose surfaces were irregular, the one being on the right side and the other on the left, and not precisely in corresponding points.

The prominence alluded to, commonly commenced under one or other clavicle, and extended downwards to near the mamma, or even a little beyond in some cases, its breadth being three or four inches. It was impossible, with a little attention, to have mistaken this prominence, when it was well marked, for an original defective organisation, since both the ribs and the intercostal spaces contributed equally to form it. It was likewise impossible to have attributed it to a pleuritic effusion, because the dilatation of the chest, which sometimes accompanies the latter, is general, and also more marked below than above. It could not, moreover, have been confounded with that which results from a collection of liquids, except when it existed over the præcordial region, and was limited to it, which occurred in three cases, and in which it might have been asked whether it was not owing to an effusion into the pericardium. But the absence of the other symptoms of pericarditis did not allow us to remain very long in error upon this point. The prominence which is consequent upon an aneurism of the aorta is generally more circumscribed and better marked; so that the observation merely of the prominence which we have described, of its form and extent, has pointed out to us almost certainly the existence of an affection of the chest, which was neither phthisis, nor dilatation of the bronchia, nor pleurisy, nor aneurism of the aorta, nor even a pericarditis (I except the few cases in which there might, at first, be some doubt upon the subject). The dyspnœa, too, having been present for a certain length of time in these cases, and with the characters noted in the preceding paragraph, we are obliged to consider them as so many examples of emphysema of the lungs.

Three times only out of six cases, where the back was examined, was a prominence, similar to that which has been described, found posteriorly. The preference which this elevation appears to have for the anterior part of the chest, is in accordance with what we have already stated as regards the lungs, viz., that their free border and the neighbouring parts were the principal seat of emphysema, and the point where the latter was most developed.

Further, the prominence we have been speaking of was not equally frequent on both sides; it occurred eleven times only on the right, and twenty-three times on the left. Notwithstanding that the numbers are small, it is not easy to suppose that the above difference is altogether accidental, since we arrive at a similar

result, or thereabouts, by consulting separately the cases recorded by Jackson and myself. In fact, the proportion of the elevations on the left side to those on the right, is 15.7 for the one, and 8.5 for the other.

This result, however, is, to a certain extent, in opposition to one which was announced in the chapter on pathological anatomy—viz. that both the amount and frequency of emphysema appeared to be the same in each lung. But may it not be that the unequal frequency of the elevations on the left and right side, among the patients of the first series, was temporary, and that it disappeared in proportion as the disease became more advanced? By this supposition, the above contradiction would only be apparent, and opposite results would be harmonised. But it is merely a supposition, and experience must determine the question.

In emphysema, there is also found another prominence, different from the one above described, and which I have lately observed, occurring in a different part of the chest, behind the clavicles. This elevation existed in every case where it was looked for, with one exception, which presented itself in a thin man aged forty-three years, and in whom the supra-clavicular regions were both hollow, although there can be no doubt of the existence of emphysema in his case, there being a slight prominence anteriorly on one side.

This last mentioned elevation, the presence of which might have been readily inferred from that of the other, was mostly confined to one side, or that upon which the first existed. The only exceptions to this were found in four cases, where the emphysema was remarkably developed, and where both supra-clavicular regions were manifestly less depressed, or more prominent, than in the natural state, though in different degrees. The elevation now alluded to was especially observable in old men, in whom the side of the neck corresponding to it, in cases where the change of form was considerable, appeared, if we except the flaccidity of the integuments, like that of a young man; presenting a convexity superiorly, whilst on the opposite side there was a more or less decided depression.

This double elevation, which is found only in emphysema, has enabled me to recognise this affection in more than one case, where subsequent examinations have confirmed the first diagnosis.

2. *In those who died*, the form of the chest was attended to, and in every one it was found to have suffered some change anteriorly. In seven, the prominence was simple—*i. e.* limited to one side. It was present both at right and left in the eighth part of the cases, and at different points.

Three times out of seven it was remarked on the left side—a result the inverse of what was found in those who left the hospital more or less relieved, and which is to be accounted for, no doubt, by the smallness of the numbers; for in the patients of the present series, when regarded anatomically, and in greater number, both lungs appeared to be equally affected, or thereabouts. Further, in the class of cases now under examination, the greatest development

of the emphysema corresponded to the prominence, or prominences, indicated; so that it is impossible to have any doubt as regards their importance, or the cause producing them, in the disease before us.

As for the supra-clavicular regions, they were more prominent on one side than the other in the three cases where they were described, and, as in the patients of the preceding series, this elevation was found on the same side as that which had its seat on the anterior portion of the chest, or on the side where the emphysema was most marked.

The following case is so remarkable an example of the connection which exists between the greatest degree of emphysema and the elevation of the chest in the same individual, that I shall introduce it here.

CASE III.

Frequent cough from infancy, dyspnœa augmented within the last fifteen years: during the latter period, constant cough and expectoration; palpitations at intervals for the last six years; several attacks of apoplexy during the last four, &c., &c.—Lungs prominent, their vesicles dilated—Heart hypertrophied and dilated—Partial softening of the brain—Marks of hemorrhages in the right hemisphere, &c.

A gilder of metals, aged 77 years, with a constitution originally strong, hair gray, and eyes blue, was admitted into La Pitié the 16th of April, 1832, and carefully examined upon the 29th of December and 26th of January following. Regular in his conduct, having had, at the ages of 22 and 40, respectively, a mercurial tremor; he worked but little in mercury during the interval between these periods. During two consecutive years, his 72d and 73d, dysury, passage of gravel, and even small calculi, by the urethra, after which the urine passed freely. Three days after the revolution of July, attack of apoplexy, with hemiplegia of left side. In less than six weeks afterwards the hemiplegia disappeared almost entirely, but still the side affected was never entirely restored to its natural condition. In March, 1832, there was a fresh attack on the same side, which necessitated a residence of only a week at La Charité. However, at the time of his entrance into La Pitié, the patient could not walk, and it was not till some time afterwards that he began to support himself upon his limbs. From his infancy he had been subject to short breath, and to frequent colds during winter; for the last fifteen years constant cough and expectoration, respiration more difficult than before; for the last eight or ten years has been obliged to recline with his head elevated, has had *paroxysms* of dyspnœa, and during the last five or six years has experienced occasional palpitations, which were slight, however, except after a fit of coughing or violent exercise. For the rest, he never had œdema of the lower limbs or swelling of the belly; has had no pains in his chest, except about two years ago on the left side, accompanied with bloody expectoration and fever, for which he was bled several times.

27th of December. Cough and dyspnœa moderate; percussion of the chest sonorous anteriorly, except over the præcordial region, in

a very limited space, more so behind on the right side, in the inferior half, than at left. In the lower half of the right side posteriorly the respiration is feeble, and mixed with sonorous râle; whilst in the left it is, on the contrary, rather stronger. Impulse pretty strong at the præcordial region; pulse regular, 72; a little stronger at right than left; skin cool; marked debility of the left limbs, the lower one of which drags, but is not appreciably thinner than that of the opposite side. Commissure of lips deviate a little to right side; appetite good, digestive functions natural. The patient remains almost constantly sitting near his bed, without quitting the ward.

Along with the cold weather in the first part of January, the expectoration increased, the dyspnœa remaining the same; debility more marked.

28th. After a chill which took place last evening, tongue whitish, thirst, appetite less; pulse 84; oppression of breathing increased; expectoration demi-opaque, with somes streaks of blood; chest contracted and flattened at left, except in a very circumscribed space, a half an inch beneath the clavicle, where there is a prominence. On the right side the thorax appears convex, and presents near the sternum, from the third to the fifth ribs inclusive, a prominence about two inches in breadth, as well marked as the one above mentioned. The percussion is more sonorous anteriorly, even in the præcordial region, than natural; is not decidedly more so at right than at left, but is greater over the elevations spoken of than elsewhere, and especially over that which is situated on the right side. The respiratory murmur is very feeble anteriorly opposite the prominences mentioned, especially opposite the right one, where in fact it is scarcely heard, and in the lower half of each side. Behind, where it is not easy to institute an examination on account of the weakness of the patient, the pulmonary expansion is feeble on both sides, in the upper half; beneath is heard a subcrepitant râle in the midst of a feeble respiratory murmur. The impulsion of the heart is constantly strong, and is felt under the left clavicle.

29th. Pulse 120, very irregular, strong at intervals, then extremely feeble and difficult to be felt; pains beneath the left mamma, augmented by percussion, which the patient has felt occasionally for two years past, and attributed by him to a blow received at that time in the præcordial region. The impulsion in this point, the conformation of the chest, the respiration and the râle, the same as last evening; countenance more pale, appetite less, sweat at night as on the 28th.

On the 30th, the condition of the patient had not materially changed. He was bled from the arm eight ounces on the 31st, and from the 2d to the 10th of February, the day of his death, he was in the following condition.

The oppression was always considerable, especially in the evening; the respiration in the morning being 36, and in the evening 40 in the minute. A sonorous râle was always found at the anterior part of the chest, and on the 2d there was still heard at the

posterior inferior portion of the right side, a subcrepitant rhoncus. On the 9th the expectoration was opaque and a little streaked with blood.

The pulse was very variable, both as it regards its frequency and rhythm, so that on the 3d it was 100 and regular, on the 4th 86, the 5th 76, the 6th 144 and very regular, the 7th 84 and regular, as previous to the 6th, the 8th 133, small and irregular, the 9th 84 and regular, the 10th 120, very small and very irregular.

On the 2d, the right pupil was twice as large as the left one; the sight nevertheless being equally good on both sides. On the 8th, frontal pain at right; the 10th, at one o'clock in the afternoon, it exists also slightly at left, but is not exactly limited. Countenance approaching a violet colour, features sunk, whole body cool, the memory and the other intellectual faculties in good condition; the dilatation of the right pupil continues; the left arm, which has become gradually more feeble, and has been the seat of prickly sensations for the last five days, is immovable, and the fingers of the corresponding hand can scarcely execute a few slight movements; those of the leg are free, although feeble, and this limb, like the forearm of the same side, is a little infiltrated.

On the 7th the patient still took a little soup, urination began to be difficult, and continued so afterwards; the thirst was variable; no nausea or vomiting.

Death took place at 5 o'clock in the evening, three hours after the last interrogatory.

The patient was bled a second time on the 9th, without any evident melioration, and his blood, which coagulated feebly, was covered with a red crust of no great thickness.

Autopsy thirty-nine hours after death. Exterior condition.—Same infiltration as during life; some black spots upon the right haunch, not penetrating more than half way into the thickness of the skin; no stiffness of the limbs.

Head.—A small quantity of blood on the external surface of the dura mater, in which is an ossification near the falx, of about an inch in length by three lines in breadth. Considerable infiltration under the arachnoid, especially behind; arachnoid opaque and thickened as well as the pia mater. Hemispheres of the brain apparently smaller than common, but perfectly equal. Cortical substance very pale; white substance but little injected, of good consistence. An ounce of clear serum in each lateral ventricle; right corpus striatum a little larger and less consistent than the left; vessels on its surface far better marked than those on the latter. Pretty close adhesions between the right corpus striatum and the demi-transparent septum, situated about a line below the corpus callosum, and extending for an inch in length by two lines in breadth; septum lucidum thicker and firmer there than elsewhere, also a little yellow. In the interior of the right optic thalamus, about six lines from the tubercula quadrigemina, and seven from the corresponding ventricles, was a small cavity nine lines in length by two in breadth and about one in height. This cavity was

empty, and lined by a thin yellow false membrane, giving rise on its internal surface to little filaments which might be seen floating about when immersed in water. Two or three lines behind this cavity was found another, much smaller, but otherwise similar to it, whilst a little more to the right was observed a third one, very small, of the size of a little pea, containing coagulated blood, and lined by a thinner and more polished membrane than that of the others. Around these cavities, for a short distance, the cortical substance is a little less firm and less white than elsewhere. In the left lobe of the cerebellum, posterior to and partly in the corpus rhomboideum, was also found a small cavity, almost effaced, four lines in length and a little less than one in breadth, lined by a membrane similar to that which has been described, of a brownish and grayish colour, and tolerably firm. The tissue surrounding it, like the rest of the cerebellum and the annular protuberance, appears in the natural condition.

Neck.—Epiglottis and larynx perfectly healthy; some pretty viscid mucus in the trachea, the mucous membrane of which, of a light red, is of the natural consistence and colour.

Chest.—Right lung large, not collapsed, extending beyond the median line to the left, especially in the middle portion; of a pale gray, presenting some slight adhesions laterally and between its lobes, which are four in number (the fourth is almost triangular, very small, and behind the middle one). Its free border is less sharp than common, its vesicles every where dilated, but unequally, more so at the base and along the free border than elsewhere, the largest being about the size of a pin's head. At the summit, for four or five inches in extent, the pulmonary parenchyma contains a red, abundant, spumous liquid; elsewhere it is pale, and has but little blood. The bronchial tubes of the lower lobe are red, evidently thickened, less transparent than usual, and contain a great deal of mucus. Those of the upper lobe are perfectly healthy, except only a single one, which is obliterated at three quarters of an inch from the surface of the lung, and has around it, at that part, an irregularly rounded, hard, white mass of the size of a pea, which creaks under the scalpel. The left lung is small, does not pass beyond the part where the cartilages and ribs unite, and adheres throughout its whole extent to the costal pleura, by means of a false membrane a line in thickness, and infiltrated with a yellowish serum. Its vesicles are every where dilated, although less so than those of the right lung, except at the summit, where they are as big as the head of a large pin. Its parenchyma, like that of the right, contains a considerable quantity of red spumous liquid, without induration. The upper bronchial tubes are a little thickened, and contain a considerable quantity of mucus; no effusion of liquid into the corresponding pleura; an ounce of limpid serum in the pericardium. Heart large; its left ventricle is almost as broad at top as bottom, is four inches long by four and a half broad, and has walls nine lines thick in the middle. Towards its point it presents internally a sort of cul-de-sac, the walls of which are thinner than those of the sur-

rounding parts. The fleshy fibres of this cul-de-sac are of a reddish brown, both upon the internal and external surface; and between these two layers is found another of a yellowish colour like the aorta, evidently softened in the centre. In this cul-de-sac, also, the internal membrane is not found, and at its circumference the fleshy columns are half broken. Further, there was found, in the whole anterior portion of the ventricle, a yellow layer more consistent than the healthy parts of the heart, without any distinct fibres, a little softened, and not greasing paper. The mitral valve presents, for twenty lines in length, an elevated border, three lines thick. The right ventricle is a line and a half thick at the middle portion; nothing remarkable observed. The aorta is three inches in circumference at its origin, and is studded with numerous yellow spots and ossifications throughout its whole extent. The circumference of the pulmonary artery at its origin is only thirty-four lines. The posterior and anterior tibial arteries of both sides are ossified, and completely obliterated about the middle of their course.

Abdomen.—Pharynx in a natural condition. Epithelium of the œsophagus a little thicker and more easily raised than in the healthy state. Stomach tolerably large, containing a small quantity of mucus partly tinged with blood. Its mucous membrane a little injected in the great cul-de-sac and on the posterior face, of a very decided gray slate colour along the large curvature as well as the anterior face and near the pylorus, mamelonated wherever this gray colour is found. The membrane is of good consistence every where except along the great curvature, where strips of only four or five lines long can be raised; thickened in the great cul-de-sac and along the anterior face, where it is three or four times as thick as on the posterior face. The duodenum was a little red on its internal surface, without other material alteration. The small intestine was larger than natural, contained a fluid more or less thin, which was alternately brownish or yellowish, of the colour of wine lees in the two last feet. Being somewhat injected in the upper half, especially in the cellular tissue, it was of a pretty deep red near the cœcum for about two feet in length; in which latter part the mucous membrane, which was of the same colour, was a little thickened, and could only be raised in strips of from one to three lines in length, whilst in the neighbouring parts these were from three to four, and, still higher up, from four to six lines long. Some crypts of Brunner were visible in the two last feet; the mesenteric glands were small and healthy. The large intestine contained substances the consistence of which gradually increased as you went towards the rectum, where they were moulded although still soft. Its mucous membrane was pale and of good consistence and thickness, except for about three inches in the transverse colon, where it was red, and gave strips of only four or five lines. Crypts of Brunner few and but little developed. Liver a little pale, otherwise healthy. The gall-bladder presented nothing peculiar, except five or six concretions of from four to six lines in diameter, yellow,

of a mamelonated aspect, and composed in a great measure of a yellow crystalised substance. Spleen of good size, covered with an envelope partly cartilaginous and irregular on the surface. Kidneys of natural size; their external surface after the removal of their coat was crimpled as it were; their vessels a little larger than common; small serous cysts were found at their surface, particularly on the left side, where were also observed, situated in the pelvis of the organ, a very large number of yellow gravel stones of the size of a pin's head and smaller. Similar ones were found in the bladder, and a small stone, six lines in length, three in breadth, and about half as thick, occupied the canal of the urethra. The bladder was small and perfectly healthy. Two hernial sacs were found extending from one end to the other of the scrotum, but containing no intestine; and in the septum which separates the testicles was observed a tumour five inches long by two broad, and about two and a half in thickness, whitish, hard, its cut surface shining, creaking under the scalpel, whilst in the midst of it were to be seen several little pockets filled with serum.

In accordance with what was stated in the general description of the disease, the right lung in the preceding case was free from adhesions, did not collapse, but was of large volume; its free border being rounded and extending a little beyond the median line. A similar condition of parts could not exist on the left side on account of the universal adhesions; but on both sides the dilatation of the vesicles was general, and we wish especially to call attention to the fact, that the maximum of dilatation corresponded to the two prominences on each side; so that no doubt can now exist as regards the importance of the latter, and their cause.

In this, too, as in many other cases, the dyspnœa was traced to infancy, but had remained stationary for a considerable length of time, not being augmented in fact until a pretty advanced age, viz. sixty-two years. *Paroxysms* of difficulty of breathing showed themselves only during the last eight or ten years; and on account of the enlargement and other lesions of the heart which were found, but which I cannot now stop to consider, the reader will perhaps ask whether they were not the real cause of the dyspnœa in the case before us. Without doubt they increased it during the latter part of the patient's life; but as we cannot suppose it to have been present previous to the occurrence of palpitation, we must conclude that nothing which occurred previous to the last six years can be attributed to disease of the heart, and that the dyspnœa which had existed from infancy, as well as the paroxysms of it in after life, must be attributed to the pulmonary emphysema.

It is also worthy of observation that although the bronchial tubes were dilated, their dilatation was partial, and no way proportionate to that of the vesicles, which was universal. Notwithstanding, too, the long duration of the catarrh, there was not the smallest trace of tubercles or gray semi-transparent granulations in the lungs. These two facts, however, are not at all surprising, as they are in accordance with a general law; but it is truly astonishing to find a man

arrived at the age of seventy-seven years who had had dyspnœa from infancy, and who, in the last five or six years of his life, had been attacked with pleurisy, organic affection of the heart, several cerebral hemorrhages, and gravel. What strength of resistance in the vital power!—what perseverance and length of time, too, it must have required to obtain from a patient, intelligent to be sure, but still one that was weak and suffering, all the details which the author of the case has left us, and which are all so well explained by the lesions afterwards found, and whose occurrence was foreseen! To what results may not observation pursued in this manner lead?

It is no doubt owing to the difficulty of examining the chest consequent upon the debility of the patient, that we must attribute the indecision of the author in relation to the relative sonoriety of the two sides of the chest. The sound on the left side ought to have been less than on the right, since the former was contracted, and the dilatation of the cells of the corresponding lung was a little less than on the opposite side.

As regards the two forms of prominence which we have been considering, I will observe that the greater their importance in reference to the diagnosis of emphysema, the greater should be the care exercised in determining their existence in individual cases, in order to avoid the mistakes into which we might fall in this respect. For this purpose, if we wish to determine the form of the chest anteriorly, the patient being in bed, he should lie upon his back and be inclined to neither side; without which latter precaution the anterior part of the chest might appear prominent when it was not really so. If on the contrary we wish to determine the form of the supra-clavicular region, the patient should sit upright, without bending to either side, the arms hanging down along the side or crossed in the same position; and if the difference between the two regions is not evident, the patient must stand up out of bed, taking care not to support himself on one hip, and with his arms placed as before mentioned. In fact it would be proper in every case, to place the patient in this position, in order to determine more accurately the various alterations of form which the chest may present.

§. III. *Of the Sonoriety of the Chest.*—1. *In the patients who left the hospital more or less relieved*, the percussion of the chest was more sonorous than natural, as has been previously mentioned, over a surface variable in extent, and sometimes over the whole chest, in cases where there was no tuberculous complication. But one exception occurred to this rule, in the case of a corpulent man of short stature and broad make, thirty-seven years of age, with a well-developed muscular system, whose chest gave but little sound throughout, in consequence, no doubt, of the great thickness of the soft parts; for in this patient, who had experienced for more than twenty years all the rational symptoms of emphysema, and who had not felt those of any other affection, except a little œdema at intervals for the last three or four years; in this patient, I repeat that

we cannot suppose the affection to have been limited to the internal surface or base of the lungs, a condition of parts which I once observed in this disease, and when percussion was of no service in the diagnosis; since the respiratory murmur was feeble throughout, and accompanied and often even masked by a whistling sound of which we shall speak in the next paragraph.

But in almost every case where the sonoriety of the chest was augmented, this augmentation was neither uniform, nor similar at corresponding points on each side, and it was at its maximum in those parts where the prominences were found. Owing to a negligence which I very much regret, and which I mention in order that others may avoid it, I almost always omitted to examine the form of the back. Eight times, however, I found excessive sonorousness behind, either at right or left, on the same side on which a prominence was found anteriorly; in which cases, perhaps, there was also one behind where the percussion was most sonorous.

The simultaneous existence, in one portion of the chest, of a prominence and a degree of sonorousness greater than that of the surrounding parts, showed, independently of any other symptoms, that the prominence was not owing to an effusion of liquid, nor, when it existed at the præcordial region, to enlargement of the heart, but to the presence of air accumulated in the part to an extent beyond what is natural. The next question to be determined was whether the air was still contained in the lungs, or was effused into the cavity of the pleura. An effusion of air could not take place except through the medium of a perforation of the pulmonary tissue; and the symptoms of this perforation, which are connected almost exclusively with phthisis, not having taken place, we might, by means of this negative fact and independently of auscultation, arrive at the conclusion that the prominence and excessive sonorousness were owing to a considerable dilatation of the pulmonary vesicles. But the emphysema is never limited to the elevated portion of the chest; so that the simultaneous existence in one part, of these two symptoms, viz. an elevation and excessive sonorousness, point out with almost positive certainty, and independently of other signs, the existence of a more or less considerable and extensive emphysema of the lungs.

2. *In the patients who died*, the same excessive sonorousness existed in analogous points, that is, in those which were the most prominent and corresponded to that part of the lung where the vesicles were most dilated and contained the largest proportion of air. (Case 3.)

§ IV. *Respiratory Murmur, Ronchi.*—1. *In the patients who left the hospital more or less relieved*, the respiratory murmur was feebler than natural, either throughout the whole chest, which was rather uncommon, or in one portion only. No where, however, was it so feeble as under the elevation so often spoken of, with one exception, which it will be easy to account for after reading the account of the fourth case. This diminution of the respiratory murmur was the more remarkable in five cases, inasmuch as it was

confined almost entirely anteriorly to the part of the chest which was elevated, and to the corresponding portion behind. On this account it could be attributed only to the dilatation of the air-cells, that is, to the emphysema.

This feebleness of the murmur remained the same, or nearly so, during the whole time that the patients remained in the hospital; if we except one case where this symptom diminished very much without entirely disappearing. In four cases, also, the respiratory murmur, without seeming decidedly feebler than natural opposite to the prominences, was a little hard, and as if it were produced by the air entering into cells which were smaller than those on the opposite side, where the respiration seemed finer and softer. These sensations, which perhaps will appear obscure on account of the difficulty of finding terms by which to define them properly, these sensations, I repeat, were nevertheless very well marked and easily appreciated. Several times within the last two years, and since the facts which I am analysing were recorded, I have had the opportunity of again observing them in analogous cases, in the presence of other individuals. How shall we account for them? As the condition of the lungs could not be determined by a post mortem examination, perhaps it will be said that there was in these cases a greater or less dilatation of the bronchial tubes. But this supposition cannot be admitted, because broncophony was absent in every case; and on this account it is extremely probable that the roughness of the respiratory murmur depended both upon the hypertrophy and dilatation of the pulmonary vesicles. These, too, being less numerous in the part alluded to than in the corresponding portion of the opposite side, would make the murmur seem more scanty than natural. However, this view of the subject cannot be regarded as correct, until it shall be verified by a greater number of facts.

Although the feebleness of the respiratory murmur was generally proportionate to the duration of the disease, it is also true that there were some remarkable exceptions to the rule, especially in the cases of three individuals, whose ages were severally eighteen, thirty, and forty-eight, who had laboured under the symptoms of emphysema only for two and a half years, and in whom, nevertheless, the respiratory murmur was more feeble opposite the prominences, than it was in the same part in other patients who had been subject to the same symptoms for more than ten years. This fact might have been deduced from that previously established, viz., that emphysema is sometimes slight in patients who have died when upwards of sixty years of age, and who have been subject from infancy to a greater or less degree of dyspnœa.

Besides this feebleness of the respiratory murmur, one or two species of râle were heard in every case. The one, the sibilant and sometimes sonorous râle, was found in thirteen of the thirty-one cases which were examined with sufficient care in reference to this point. This râle existed in various degrees, was more marked at the time the patients entered the hospital than when they left it, and most commonly was nearly universal. In four cases, how-

ever, it was limited to the prominent portion of the chest, or in other words to that part of the lungs where the dilatation of the air-cells was the greatest. It would thus seem that this râle, although it may be found in other diseases, is perhaps especially connected with emphysema.

In four cases there co-existed with the above râle another sound of a very different character, I mean the subcrepitant râle, which was found also in eighteen other cases, and in both was situated behind and inferiorly, and mostly on both sides at the same time. I have observed but one exception to this rule, and that was in the case of a patient in whom the subcrepitant râle occupied the whole height of the chest posteriorly, and merely a small portion of the same side in front under the clavicle. This exception, however, was more apparent than real, because the râle had still commenced at the base of the chest, and had not arrived at the summit until after the lapse of several days, and besides, its disappearance first commenced at the latter point. We may add, that in no case did this râle exist at the anterior part of the chest, opposite to the prominence. This absence of the subcrepitant râle opposite that portion of the lungs where the emphysema was most developed, does not prove that it cannot exist where there is a certain degree of dilatation of the vesicles, especially as in several cases where this râle was present posteriorly, there was certainly at the same time a greater or less dilatation of the cells in the same part. But the majority of those who came to the hospital with the symptoms of emphysema, had been labouring for some days previous under an acute pulmonary catarrh, which had very much augmented their habitual dyspnœa. Now, subcrepitant rale is one of the most common symptoms of acute pulmonary catarrh, and when it is so, it is uniformly, and without exception, found (according to my observation, and I have latterly tested its correctness in a hundred cases) at the posterior inferior portion of the chest and commonly on both sides. This remark holds good, not only where the pulmonary catarrh is simple, but also where it occurs as a complication in another disease, as typhoid fever; and even, too, where there is something specific in its character, as in measles. It is not astonishing, then, that the seat of the subcrepitant râle should still be the same in the cases before us. This law of its development is further important, inasmuch as it seems to indicate that emphysema, the maximum of which is usually found near the free border of the lungs, is independent of pulmonary catarrh, at least when of an acute character. Besides, it augments the number of facts, already very considerable, which show the complete distinction which exists between catarrh and tubercles.

However, this subcrepitant râle does not appear to me to differ from that which has been mentioned by the illustrious author of auscultation, as pathognomonic of emphysema of the lungs; and the reader, finding before him two such contrary assertions, will no doubt enquire which is true. But his hesitation will quickly vanish, and he will repel the assertion of Laennec, provided he does not lose

sight of the facts above mentioned,—that the subcrepitant râle of emphysema was the same, followed the same laws, and was found in the same parts, as that which is connected with acute and severe pulmonary catarrh; and that it was not heard in those portions of the chest which corresponded to the maximum of the emphysema. It is, moreover, to be regretted that Laennec has not pointed out the parts of the chest where he observed this râle, which is noted in only one of his cases and that vaguely; for no doubt they were the same as those in which I observed it, and the conclusion from the facts which I have brought forward would probably have appeared more evident. How great the precision, then, and how numerous the details, which it is necessary to introduce into the histories of particular cases in order to avoid error, and arrive at a knowledge of the laws of our economy!

2. *In twelve of the patients who died*, who were auscultated and in other respects carefully examined, the respiratory murmur was very feeble, and especially at that part of the chest where there was a prominence, and where the maximum of dilatation of the air-cells existed.

In one case, where the form of the chest was not described, (case 1), the respiratory murmur was absent, or nearly so; and in several parts there was heard, during inspiration, only a more or less prolonged whistling sound. The reader will no doubt recollect that this case was one of all others in which the dilatation of the cells was carried to its greatest height. We cannot doubt then that the prominence of the chest, its excessive sonorousness in the same part, as well as the feebleness of the respiratory murmur, are connected with an anormal and more or less marked dilatation of the pulmonary vesicles.

The following case appears to be an exception to this law; but the exception is only apparent, and is explained by the disposition of the parts.

CASE IV.

Cough and oppression for more than four years—A prominence upon the anterior and posterior portion of the left side of the chest; sonoriety greater than common on both sides in different points; diminution of the respiratory murmur in some parts—Lungs rounded at their free border, not sunken; their cells universally and unequally dilated—Some of the bronchia a little thickened, without dilatation.

A seller of lemonade, aged seventy-two years, with but moderate intelligence and a poor memory, was admitted into the hospital of La Pitié the 18th of January, 1833. When questioned at several different times, from the 19th to the 25th, he always replied that he had coughed and been subject to oppression ever since the revolution of July, that he had experienced some dyspnœa anterior to this epoch, but not so far back as infancy, and that he could not recollect accurately the time of its commencement. He added that he was subject to palpitation, and that he had never spit blood or had any swelling in his lower limbs. At several different times, also, during the same period, from the 19th to the 25th, there was

found a subcrepitant râle at the posterior portion of the chest on both sides. On the 25th a very attentive examination of the chest and of the state of the respiration enabled us to determine the following points. On the left side, under the clavicle and posteriorly, opposite the angle of the ribs, there were prominences which did not exist on the right; percussion throughout very sonorous, a little more so for an inch beneath the left clavicle than at the corresponding part on the right side; beneath this point, on the contrary, it was more sonorous at right than left, whilst posteriorly it was the reverse; between the left nipple and the median line, for the length of about two inches, there was a little less sound than elsewhere. On the right side, the respiratory murmur was less feeble under the clavicle than on the left; below, all around, for two or three inches in height, it was more feeble than above. On the left side the feebleness of the respiratory murmur increased as you descended from the clavicle to the nipple, an inch beneath which it was not heard. It was very feeble under the arm-pit and in the upper three inches behind, more so even than at right, and beneath it was not heard at all. There was no sound on expiration, or resonance of the voice in any part. A little subcrepitant râle behind at the lower part of the right side, and a little sibilus above. On the left side there was a universal subcrepitant râle, but much less numerous anteriorly than posteriorly. Impulse of the heart strong; its pulsations heard throughout the whole chest. Dyspnœa considerable; head very much elevated.

From the 25th to the 29th, no appreciable change. On the 29th, the signs from auscultation and percussion were the same as on the 25th; dyspnœa constantly great; respiration thirty-six; cough very frequent, causing pain on the right side; the expectoration scanty, there being in the spitting-cup but one sputum, which was greenish, opaque, and not aerated; pulse 130; deglutition easy; tongue violet coloured and moist; no thirst; appetite moderate; tendency to stupor. Patient has lost flesh decidedly since entering the hospital, without having had diarrhœa.

On the 31st, at four o'clock in the evening, cough exceedingly frequent; respiration thirty-six; pulse 110, very irregular.

1st February, nearly at the same hour, dyspnœa is a little less; respiration only twenty-four; expectorated matter, consisting almost entirely of frothy saliva, in the midst of which are some greenish masses, opaque and not aerated; pulse 108.

2d, at ten o'clock in the morning, countenance of a livid red; heat augmented; crepitant and subcrepitant râles posteriorly and inferiorly on both sides. (v. s. ʒx.)

In the evening, orthopnœa; respiration forty-eight; speech interrupted and almost impossible; two or three demi-transparent expectorated masses, a little rusty and viscous; countenance violet coloured, pale, and livid; pulse 144; tongue clean, moist, violet coloured; one natural stool; answers correct; abdomen free of pain. Some hours afterwards the patient was bled, but without

relief, and died about eleven o'clock in the evening. The blood was not covered with a crust.

The examination of the chest, repeated several times after the 25th of January, always gave the same results.

Examination of the body thirty-six hours after death. Exterior condition.—Paleness, without lividity; slight stiffness of the limbs; second stage of emaciation.

Head.—Some drops of blood on the external surface of the dura mater; glands of Pachioni pretty well developed, chiefly behind; slight infiltration under the arachnoid; scarcely a small spoonful of clear serum in each lateral ventricle; a slight lilac shade in some parts of the white substance. Nothing else worth observing in the whole brain.

Neck.—The pharynx, epiglottis, and larynx, are in a natural condition; trachea red, its sub-mucous cellular tissue injected, its mucous membrane otherwise perfectly healthy.

Chest.—The pleuræ contain no serum. The lungs pass the median line; the right one has some cellular adhesions in its two upper thirds, except anteriorly and in the fissures; its size is considerable; it does not sink on opening the chest, and at its summit it is overlapped by the left lung, for about an inch. It is light, soft, and pale, except inferiorly and posteriorly, where it is a little heavy, non-elastic, and of a violet red in its lower half. Its free border is rounded, turgid as it were; its middle lobe overlaps by half an inch the summit of the inferior, which is twice as large as the corresponding one on the left side. Its cells are dilated throughout its whole extent, a little less at the summit than elsewhere, the dilatation becoming greater from behind forward, and the more so as you approach the free border. The dilatation is greater along the lower border and inferior surface of the middle lobe than in any part of the superior lobe; it is still more so along the anterior and sharp edge of the inferior lobe, which at this point is eight or ten lines thicker than natural, and demi-transparent—the vesicles in the same part being twelve or fifteen times their usual size. The dilatation continues to be apparent until you reach that part where the engorgement above mentioned exists. The parenchyma of the superior lobe contains only a small quantity of a pale and spumous liquid. A part of the middle and almost the whole of the inferior lobe contain a greater quantity of liquid, which is red and almost throughout spumous. A portion of this lobe, about an inch and a half in extent, has a granulated aspect, is of a deeper red than the surrounding parts, contains but little blood, is destitute of air, and sinks to the bottom in water. The bronchial tubes contain a greater amount of mucus the lower you descend; they are no where enlarged, and seem merely to be a little thicker in the middle and inferior lobes than natural. The left lung is heavier than the right; has adhesions between its lobes, and at the inferior part of the upper one, posteriorly and laterally. Its two lobes are violet coloured behind and on the sides; their free border is more rounded than natural; that of the upper lobe is more so than the corresponding

one at right, the reverse of which is true of the inferior one. The dilatation of the vesicles, which is universal wherever they can be distinguished, is greater in the upper lobe than in the analogous one at right, and in some points two or three times greater; the reverse of which is true here also of the inferior lobe. The bronchial tubes contain a great deal of mucus; those of the upper lobe present nothing remarkable, except at the narrow border, where they are manifestly a little thickened, without being dilated; the same is true of those near the internal face of the lower lobe. The others present nothing unnatural, except a little redness in those parts where the surrounding tissue is red; the two lower thirds of the upper lobe are hepatised laterally and posteriorly, to the depth of about ten or twelve lines. In front, the pulmonary parenchyma is not granulated, swims on water, and contains a spumous liquid. Same red hepatisation of the lower lobe, but over a smaller extent. The pericardium contains an ounce of clear serum. The heart is of good volume, firmness, and colour, and has a small white spot on its surface. The walls of the left ventricle are four lines and a half thick at the summit, and five lines elsewhere; those of the right ventricle a line and a half and two lines at the same parts. A part of the internal membrane of the left cavities is slightly opaque, whitish, and thickened. The semi-lunar valves of the aorta are three times their usual thickness, whitish, and less flexible than usual; the aorta is thirty-three lines in circumference—the pulmonary artery thirty-six.

Abdomen.—A scrotal hernia on the right side contains a fold of the small intestine. Stomach pretty large, containing five or six ounces of a greenish and yellowish liquid. Its internal surface is grayish, except in a small portion of the great cul-de-sac, where there is a slight injection. Its mucous membrane is elevated, both in this portion and on its posterior face, by very turgid veins; it is of the usual thickness throughout its whole extent; gives strips of from two to four lines in the great cul-de-sac, from six to eight on the anterior and posterior surfaces, from eight to ten along the small curvature, and is no where mamelonated. Nothing remarkable in the duodenum. The small intestine is of the usual size, and contains in its upper half a moderate quantity of yellowish fluid, which becomes brownish and greenish towards the termination of the ileum, where it is of a pultaceous consistence. Its mucous membrane is pale and of the natural consistence throughout its whole extent; gives strips of three to four lines in length in its upper half, and of eight to ten in the remainder. No glands of Brunner. The large intestine contains fæcal matters, the consistence of which becomes greater as you go from the cæcum to the rectum, where they are abundant and moulded in form. Its mucous membrane is pale, of good thickness, gives strips of from two to four lines in the cæcum, from eight to ten in the transverse colon, from twelve to sixteen in the remainder. The glands of Brunner are pale, small, and numerous. The liver contains a little more blood, and is a little less cohesive than natural, without any other appreciable

alteration. The gall-bladder contains a yellowish liquid, which runs pretty freely, and is in small quantity. The spleen is two inches and a half long, by one and a half broad, and one in thickness; it is covered with a cartilaginous membrane over nearly its whole surface, smooth externally, rough internally, its maximum thickness being one line; four fifths of the organ consists of a liquid substance, of the colour of wine-lees, deposited in the midst of a reticulated tissue, which is flabby and not easily torn: the remainder is of a normal consistence. The external surface of the kidneys, when deprived of their envelope, is more or less lobulated. These organs are otherwise perfectly healthy, as is also the bladder.

Except the acute pulmonary catarrh and the partial pneumonia, evidences of which were found in the post-mortem examination, the disease of the patient before us was without complication; and there would be no deficiency in the history of the case, which is so interesting on account of the number and accuracy of its details, if the intelligence of the patient had allowed the author to assure himself of the origin of the disease, and of the fact of the occurrence or otherwise of *paroxysms* of dyspnœa. However this may be, the condition of the lungs, compared with the results of auscultation and percussion, are well worthy of attention. The cells of the upper left lobe were generally more dilated than the corresponding ones on the right side; and the percussion far from being less sonorous—the respiratory murmur, instead of being more feeble at right than left, at the distance of an inch from the clavicle, presented entirely opposite characters. However, as Jackson remarks at the conclusion of the above case, what did it require in order that the percussion should be more sonorous anteriorly at right than left, in about the upper three fourths of its height? Merely that the amount of air corresponding to it should be greater than on the left side; which in fact was the case, considering the preponderance in volume of the middle and inferior lobes over the corresponding portion of the left side. On the other hand, what was necessary in order that the respiratory murmur throughout the same extent should be a little less feeble at right than left? That the most superficial vesicles of the middle and upper lobe, those in which the entrance of air is heard, should be more dilated than on the left side in the same lobe; and this also was really the case. So that the above circumstance, which might have been considered at first sight, for want of a certain degree of accuracy, as an exception to the rule, is on the contrary a confirmation of it. Every where in the upper half of the left lung the cells were more dilated than in the corresponding portion of the right; and every where, also, the respiration was more feeble in the first than in the second. However, it cannot be doubted that the differences above mentioned, relative to the sonoriety and intensity of the respiratory murmur on each side in front, were augmented in consequence of the acute catarrh, which existed, contrary to what is usual, on the left side alone.

Further, one of the facts which deserves most particular attention

here, is, as has already been said, that there was in the above case no complication of chronic disease, or the least obstruction to the circulation; consequently, we cannot attribute the dyspnœa which the patient had experienced for several years to any cause but emphysema.

This case also is one of the most remarkable examples of what has been stated in the general description—viz., that the dilatation of the pulmonary vesicles is generally greater along the free border than elsewhere. We have seen, in fact, that in some points this border was thicker by six or eight lines than in the natural condition; that at the summit of the left lung it passed the median line, &c. It should also be observed, that notwithstanding the extent and severity of the lesion, the bronchial tubes were not dilated; that some of them appeared merely a little thicker than natural.

Pains in the Chest.—1. *Amongst the patients who left the hospital more or less relieved*, fifteen out of thirty-two who were examined in reference to this point, or about one half, had pains in the chest; and in thirteen cases these pains were situated on the side of the thorax, where the prominence so often mentioned existed. These pains, the character of which I could not determine, came on at very different periods, from one to three or four or even a greater number of years prior to the time at which the patients came under our observation. They were not increased by inspiration or coughing, and on this account could not be attributed to chronic inflammation of the pleura, as also because the seat of the pain was commonly the anterior surface of the chest, which corresponds to that part of the lungs which is found free from adhesion in emphysema, as well as in other diseases, except only where the adhesions are general. Neither could they be attributed to extension of the thoracic parietes, because this extension, when it is the result of an effusion not consequent upon inflammation, is not accompanied by pain; and as these pains had their seat thirteen times out of fifteen on the same side with the prominence, and in the part which corresponded to it, we are obliged by the method of exclusion, and, as it were, in spite of oneself, to attribute them to the dilatation of the vesicles themselves.

Although this is the only conclusion which can be drawn from the facts which I have stated, it does not yet appear to me perfectly clear; and before admitting it as rigorously established, I should like to have a greater number of cases, all of which should be carefully examined in reference to the point in question, and also some histories of fatal cases. I do not, however, make the above proposition for the purpose of surprising the reader, but to satisfy his mind; for there is complete harmony between the principal seat of the disease, its chronic course, and the duration and seat of the pain. Besides, we can hardly suppose that the excessive dilatation of the cells, which, from the size of a grain of sand, became as large as a hempseed, or a pea, or even larger, and of which dilatation there is not another example in the economy—we can hardly con-

ceive, I say, that such a dilatation should be brought about without pain, at least of an obtuse character, or without greater or less uneasiness in the part.

2. *In the patients who died*, the pain in the chest was too imperfectly studied to admit of our treating of it generally; therefore, I shall not stop to consider it.

§ VI. *Of the cough.*—1. *In the patients who left the hospital more or less relieved*, cough occurred in every case, except in one which we have already treated of, when upon the subject of dyspnœa, at various periods of the disease and of various degrees of severity, constant or almost constant in some, with more or less considerable intermission in others. In fact, it presented the latter character in nineteen out of forty-four patients in whom it was carefully studied, or rather less than half.

As to the period of its commencement, it occurred simultaneously with the oppression twenty-two times in forty-six cases where this point is mentioned; fourteen times afterwards, and ten times before. The latter cases were a little less frequent in the patients whose history Jackson has recorded, than in those which I have observed myself. This circumstance is owing less perhaps to a mere coincidence than to the fact that Jackson, having collected his cases subsequent to the period at which my attention and his own were more particularly directed to the first appearance of the dyspnœa, may have determined this point with greater precision. The latter supposition is the more probable, as the same physician found a greater proportion of cases in which the dyspnœa dated from early youth or infancy, than that which resulted from my own observations.

Moreover, it is well worthy of remark, that the cough, whether continued or intermittent, did not in any case accompany the dyspnœa at its commencement, when the latter dated from early infancy; and, on the other hand, that it did not appear subsequent to the oppression, where the latter commenced at about the age of twenty years, or later, except in a single instance. Hence it will perhaps be asked if I have not fallen into a serious error in dating the origin of emphysema from early youth, in those cases where the dyspnœa commenced at that time. But this doubt will soon be dissipated, if it is recollected that the cough was far from being constant in every case; that in one of these, even, it had not shown itself at all, notwithstanding that the most marked physical signs of emphysema had existed for some time; that hence it is not essential to the disease; that besides, the character of the dyspnœa, both previous and subsequent to the occurrence of catarrh, was always the same; and, finally, that dyspnœa is not one of the necessary symptoms of pulmonary catarrh. I will add, that Mr. Lediberder has more than once observed emphysema of the lungs upon the dead bodies of infants who had died during the first weeks or months of existence, and that, consequently, we cannot doubt the existence of the affection in the earliest periods of life.

Further, in those cases where it existed without intermission, the cough was generally very slight, except during paroxysms of

dyspnœa, which were frequently owing to acute pulmonary catarrh, and during winter.

2. *In the patients who died*, the cough followed the same course, under similar circumstances, as in those whom we have just spoken of; so that the farther we advance in the study of the symptoms, the more clearly we perceive the parallel established between the patients who died and those who left the hospital more or less relieved; which removes every doubt in relation to the character of the disease in the latter cases. In fact, out of fifteen patients who died, three had no cough till some years after the commencement of the oppression, and this latter dated from infancy. In a fourth, on the contrary, the cough—a constant cough—preceded the dyspnœa by ten years; it appeared along with it in the others, except in the case of one patient, who had had dyspnœa for fifteen years when he died; a dyspnœa which could not be attributed to an affection of the heart, which existed it is true, but the first symptoms of which appeared only two years before the fatal termination.

§ VII. *Of the sputa.*—1. *In the patients who left the hospital more or less relieved*, it was impossible to get exact information in relation to the character of the sputa previous to their admission into the hospital, so that we shall only speak here of what took place in this respect after the patients came under our own observation. In thirty-five cases the sputa were carefully examined, and the following circumstances observed. In twenty-three cases they were more or less frothy, freely aerated or liquid, and resembling a solution of gum; in the twelve others they were greenish, thick, opaque, but slightly aerated, not globular (*pelotonnés*) however, or grayish, with some streaks of blood.

Moreover, with two exceptions, these sputa were connected with two altogether different states of the lungs. Those which were opaque and more or less of a greenish hue, were found in patients affected with acute pulmonary catarrh, accompanied with subcrepitant râle; those which were frothy, freely aerated, like a solution of gum, or, as it were, semi-vitreous, occurred along with chronic pulmonary catarrh, accompanied by a whistling or sonorous râle. The two exceptional cases were those of two individuals, one of whom had opaque sputa with whistling râle, whilst the other had them freely aerated and not opaque, with a subcrepitant râle. Besides the emphysema, did there exist, in the first of these two cases, a certain degree of dilatation of the bronchia, or merely of chronic inflammation, of thickening and redness of these organs?

The greenish and opaque sputa were undoubtedly accidental; that is, the effect of acute pulmonary catarrh; the others were habitual. And if we had studied them more fully, or during the whole time that the patients remained in the house, we should most probably have observed the passage of the sputa belonging to the acute stage, into those of the chronic. However this may be, it is worthy of remark, and the fact has already been mentioned, that among the thirty-five patients of whom we are at present speaking,

one only, a woman of fifty-seven years of age, had had spitting of blood, and that to a slight degree. This woman had not had her menses since the age of twenty-seven, in consequence of a severe fright, caused by the explosion of the infernal machine, which produced a suppression of the periodical flux then upon her; and these slight spittings of blood (three or four spoonfuls in twenty-four hours) recurred three times, and only during the last three years. These moderate or rather slight spittings of blood were of themselves by no means characteristic; auscultation and percussion, carefully employed, gave no signs of the existence of tubercles at the summits of the lungs, and perhaps, in fact, they did not exist. But whatever was the cause of this hæmoptysis, its absence is not the less remarkable in the other thirty-five patients who had been subject for a longer or shorter period, the mean duration of which was ten years, to a pulmonary catarrh, which was either permanent or of frequent recurrence; for as the lungs must have been jarred in a thousand different ways during this length of time, one would say, at first sight, that they ought to have been the seat of more or less frequent hæmorrhagies.[1] What more decided proof could we have of the necessity of a special cause for the production of hæmoptysis?

2. *In the patients who died*, the sputa presented the same characters as those above described, and they were examined in such a small number of cases that we shall not speak of them more in detail.

§ VIII. *Palpitations, œdema.*—1. *In the patients who left the hospital more or less relieved*, palpitations occurred in a large number of cases, without, however, occasioning any great inconvenience to the patients, who did not call our attention to this point. Twenty out of thirty eight experienced them at various periods, and ten of these only at times when they exerted themselves very much, or when walking, or during paroxysms of dyspnœa: and it seems probable that this proportion of cases in which palpitation occurred, would be the same were the numbers increased, inasmuch as it was the same in the cases which Jackson has recorded, as in those which I observed myself.

In one of the cases where the palpitations were permanent, the latter, so far from augmenting, gradually diminished. With the exception of those patients who only experienced them momentarily, they commenced with the dyspnœa only in one instance, and in the others they came on at various periods subsequent to the first appearance of the latter symptom. Commonly, indeed, they showed themselves after the disease had run the half of its career, and even much later in some patients who were over sixty years of age, with whom the dyspnœa dated from infancy, and who had had palpitation for only a year, or even less, when they were admitted into the hospital.

[1] This woman has lately returned to the hospital, presenting some physical signs of tubercles on the right side at the summit.

2. *In fourteen of the patients who died*, careful enquiries were made in relation to the palpitations. These were wanting in four of them; were feeble and not frequent in three, and were more or less strong and pretty constant in the remainder, or about half the cases. This proportion is a little greater than that which was found in the first series. The heart, with two exceptions, was large in those who had had constant palpitations; a circumstance which no one will be surprised at, since observation has taught us long since that this is the case in a great number of instances.

In the patients who died, too, as in the others, the oppression preceded the palpitations, and frequently for a considerable length of time; so that in eight cases where the precise period at which the dyspnœa and palpitations commenced could be determined, the mean duration of the latter was seven, whilst that of the former was twenty years at the time of the patient's death; and in every individual case the dyspnœa preceded the palpitations by several years. The greater part of the patients who died, after having had constant palpitation for a certain length of time, having also had the heart enlarged, we should conclude that the same was true, under similar circumstances, of those who left the hospital more or less relieved; and the circumstances connected with the œdema, at the same time that they confirm this conclusion, point out pretty clearly the particular cases in which enlargement was really present. In fact, every patient who died after having had, for a longer or shorter period, œdema of the lower extremities, had also his heart enlarged; and, on the contrary, no one who had not œdema was found in the same condition. Hence it follows necessarily that when œdema shows itself in the course of emphysema of the lungs, we should attribute it, not to the latter disease, but to an organic affection of the heart. Among the patients who left the hospital more or less relieved, ten out of forty had œdema of the lower extremities, and always at a period long subsequent to the commencement of the disease, at a mean about twenty years. The œdema was generally slight, of momentary duration in three cases, about fifteen days in a fourth, of various degree of frequency and severity in the remainder, without, however, its having been constantly present from the time it made its first appearance. We may believe, then, independently of the symptoms derived from auscultation and percussion, that in the six last cases at least there was a certain amount of dilatation of the cavities of the heart; and we may do so with the more confidence, inasmuch as palpitations of longer or shorter continuance were found in every one of these cases.

It is easy to conceive, moreover, the slight utility of percussion when employed to discover diseases of the heart in such cases as those above mentioned, where the sonoriety of the chest is generally greater than natural, and often more so at the præcordial region than elsewhere, at which part, too, we frequently find a partial prominence.

§ IX. *Of the appetite and flesh.—In the patients who left the hospital more or less relieved*, as in those who *died*, the appetite was only diminished for a short time, as we have already stated in the general description, during a paroxysm of dyspnœa, when there was an acute pulmonary catarrh of greater or less severity. I have found but one exception to this rule, and that was in the case of a man sixty-four years of age, who had had dyspnœa from infancy. In him the oppression had very much increased within the last ten years, whilst, on the contrary, the appetite had very much diminished; the former promptly abated soon after his admission into the hospital, whilst the local symptoms of emphysema remained, and the appetite returned in the same proportion. So that in fact this patient, in whom there were no signs of tubercles, and who was well advanced in life, is rather a confirmation of the rule than an exception to it.

The *embonpoint* varied like the appetite, and owing to the same causes. The patient of whom we have just spoken had grown much thinner within the last ten years, during which the dyspnœa had been considerable, and he resumed his usual *embonpoint*, or nearly so, in proportion as the oppression diminished. Three patients had lost flesh pretty rapidly for three months previous to their admission into the hospital, although the dyspnœa was not great. These three were tuberculous. Hence it follows that if we observe a considerable and rapid loss of flesh in an individual affected with emphysema, where the dyspnœa is moderate, and the habitual pulmonary catarrh has not very much increased, we ought to suspect the presence of tubercles, of which, no doubt, a further examination would confirm the existence.

ARTICLE III.

OF THE DIAGNOSIS.

The details which we have given, when treating of each particular symptom, leave but little to be said in relation to the diagnosis; or rather, these details seem to me to establish it clearly. In consequence of the importance of the subject, however, it will be better perhaps to recur to it; and in order to render an acquaintance with it more easy, I shall place before the reader a summary view of emphysema, which may be thus defined:—

A disease without fever, of long duration, which often commences in early youth,[1] very rarely after fifty years of age, by a slight dyspnœa, which commonly continues without aggravation for a great number of years, when it dates from infancy, afterwards becoming successively more and more marked, and accompanied with paroxysms, during which the patients appear sometimes to be menaced with suffocation. This dyspnœa is frequently preceded by cough, and almost always accompanied, at some period or other of its course, by pulmonary catarrh, which, when aggravated, is

[1] This fact has been mentioned by Laennec.

one of the most common causes of *paroxysms* of dyspnœa. Connected with these symptoms we find an alteration in the form of the chest, which alteration is generally limited in extent, and consists in a prominence in which both the ribs and intercostal spaces are equally interested, and the most common seat of which is the anterior part of the chest and the supra-clavicular regions. In these elevated portions, the percussion is more sonorous and the respiration more feeble than in the natural condition, and in the other parts of the chest. A sibilant (*sifflant*) or subcrepitant râle is often mixed with the respiratory murmur. In some patients, and at a more or less advanced stage of the disease, we find palpitation, with œdema of the lower extremities. There is no loss of flesh, except when tuberculous disease is also present, or during the course of acute catarrh, which frequently complicates the principal affection, or when the dyspnœa from any cause is great, and continues so for a length of time. Finally, when the patients die, we find, upon opening their bodies, a greater or less dilatation of the pulmonary vesicles.

It is clear that a disease which presents such a group of symptoms cannot be mistaken for a chronic pulmonary catarrh; for, as I remarked above, the latter does not give rise to paroxysms of dyspnœa, to prominences of the chest, to *constant* diminution of the respiratory murmur. Neither can we confound it with dilatation of the bronchia, in which the respiration, instead of being weaker, is stronger, throughout a certain extent, than in the natural condition—bronchial as it is called—and the voice resonant. It cannot be mistaken for a tuberculous affection, since, instead of a flat sound in one part, as in the latter, the sound is clearer than common, and there is no loss of flesh, except in the circumstances mentioned; neither is there any febrile movement. It is distinguished from aneurism of the aorta, or any tumour which might compress the trachea or a large bronchial tube, by the dyspnœa in these cases being more severe and more constant, and generally accompanied by a whizzing sound, which does not occur in emphysema. The sound of the chest, too, so far from being clearer in one part than in the natural condition, is frequently more or less obscure, and we hear in a certain number of cases a double anormal sound, &c. &c. It does, to be sure, sometimes happen, and a case of this kind came not long ago under my observation, that in an aneurism of the aorta which was not prominent, and compressed the trachea, and, doubtless, also one of the large bronchial tubes, the respiration was absent, or nearly so, in a considerable part of one side of the chest where the percussion was also very sonorous; both of which circumstances are observed in emphysema. But there was no corresponding prominence, so that, with a little attention, error was impossible; and if such a case should present itself, it ought not to be confounded with emphysema.

With the exception of the palpitations and œdema, which indicated complication of disease of the heart, the other symptoms were

present in almost every case whose history has been recorded by Jackson and myself; and in the small number of exceptional cases, the existing symptoms were sufficient to dissipate every doubt in relation to the existence of emphysema. In fact, in the only one where dyspnœa was absent, the chest presented a prominence, accompanied by excessive sonorousness and considerable diminution in the force of the respiratory murmur, in the corresponding part. In the one where the thorax did not present a partial prominence, the respiration was throughout very feeble, and mixed with sibilant (*sifflant*) râle, the dyspnœa had existed for twenty years, and the paroxysms of dyspnœa, which were pretty severe, had commenced at a period long anterior to the occurrence of symptoms of disease of the heart. In the case where cough was not present, as all the other symptoms existed, there was no difficulty. But there would be real difficulty, if the emphysema was limited to the internal surface or base of the lungs, and we could observe neither prominence on the surface of the chest, nor decided diminution in the force of the respiratory murmur—a case of which kind came under my observation three years ago. Under these circumstances, in fact, the only symptom which will enable us to determine the existence of emphysema is the dyspnœa; but it is necessary that this dyspnœa should present all the characters mentioned when treating of this symptom, a slow course, more or less frequent paroxysms, &c. &c., and, besides, that it should not be accompanied by symptoms of aneurism of the aorta, or by those of disease of the heart. If, however, the latter did not show themselves till long after the commencement of the dyspnœa, we must still regard the case as one of emphysema of the lungs, as was done in the instance I have just mentioned.

It will be thought, perhaps, that we insist too much upon the difficulties of the diagnosis of emphysema—upon the means of distinguishing an extremely chronic disease, and which most commonly abridges but very little, perhaps, the term of existence. But in the first place, emphysema, in a certain number of individuals, seems to have an evident influence upon the production of diseases of the heart; and further, its symptoms are sometimes very severe, and then, as in all such cases, it will be confessed that it is important to know what disease we have to treat. This is especially important in order that we may not confound emphysema, when combined with pulmonary catarrh, with a disease incomparably more serious, and that is phthisis, as has happened to practitioners who were truly skilful, but at the same time but little acquainted with the disease before us; for what a wide difference there is between the prognosis of the two diseases, and, to a certain extent, between their treatment! Thus I have seen patients sent from the north into warm climates for an affection which appeared to their physicians to be a commencing phthisis, and who were, in fact, affected with only a slight degree of emphysema, combined with habitual pulmonary catarrh. I attended two young men who had been subject to cough and dyspnœa for several years, whose

friends were exceedingly anxious on their account, and who also were, in fact, merely affected with emphysema. Another circumstance, equally important, requires that pulmonary emphysema should be well understood, and that its diagnosis should be well established; which is, that, on account of its extremely chronic course, it complicates a great number of diseases, and hence, if a person is ignorant of its symptoms, he might and would refer some of them to the intercurrent affection, and consequently commit serious errors. This is especially unavoidable as regards chronic affections which are accompanied by dyspnœa, particularly if these affections, like organic diseases of the heart, are influenced in their production by emphysema; in which case, undoubtedly, there would be attributed to the disease a degree of difficulty of breathing which does not belong to it, and its commencement would be dated from a period much too remote. Undoubtedly many histories of organic diseases of the heart are of this kind, and I do not except forty-five of this sort, which I collected with care at the time that I devoted myself entirely to observation, because doubts, and probably legitimate ones, arise at present in my mind, whether in some cases, where the disease appeared to be simple, it was not, in reality, complicated—whether the dyspnœa was studied with sufficient precision, and the period of its commencement fixed with certainty. So that, in reality, diseases of the heart should be studied, at the present day, in all their forms; in which study, the history of particular cases of those diseases heretofore published can be of but little service.

Again, even when emphysema is slightly marked, and is complicated with another disease, it is always possible, with a little attention, to recognise it. The following case is a proof of it.

CASE V.

Dyspnœa from infancy; augmented during the last twelve years, and especially about two months before admission into the hospital, with loss of flesh—Chest flattened at left, prominent at right, anteriorly—Percussion obscure at left, sonorous at right—Respiratory murmur replaced by gurgling at left, very feeble at right, in the part corresponding to the prominence—Dilatation of the pulmonary vesicles of the right side—Tuberculous excavations, &c. &c.

A workman on the wharves, aged fifty-five years, tall (five feet six inches), was admitted into the hospital of La Pitié on the 20th of May, 1833. He had never left Paris from the time of his birth, except between the ages of fifteen and thirty-two, when he was employed in the army. At the age of seventeen, he had been confined to his bed for two months by a severe attack of disease; at the age of twenty-six, he was again confined to it for six weeks by a pneumonia (cough, oppression, fever, pain in the left side); from which time until lately he had no attack of severe disease. Subject to short breath, and obliged to lie with his head elevated when in bed, from infancy; his dyspnœa had increased within the last twelve or fifteen years, and for several years back he had often been

obliged to sit upright at night, in order to get breath. Besides that, he had had, for some months past, without being able to state the matter more precisely, an habitual cough, a fresh accession of dyspnœa, and, at times, pains on the left side of the chest. For two months back, the anorexia has been almost complete, and the loss of flesh considerable. For the rest, there was neither hæmoptysis, nor palpitations, nor œdema of the lower extremities, nor diarrhœa.

On the 14th of June, having for the first time, in addition to the signs of tuberculous disease, discovered emphysema of the lungs, I examined the patient with attention, says Jackson, and found him in the following condition:—Loss of flesh and debility more marked than in the month of May; deglutition easy; no pain in the throat; anorexia, thirst, nausea only after coughing; belly soft, supple, without pain; daily stools; position in bed varied; sputa abundant, yellowish, opaque, distinct; respiration thirty; chest flattened anteriorly at left, especially above the nipple; prominent, on the contrary, at right, at the latter part, principally on a line with the junction of the ribs and their cartilages; percussion flat at left, anteriorly and posteriorly, less completely so below than above, also at right, under the clavicle, becoming less so as you descend; below the nipple indeed, and especially opposite the prominence mentioned, the percussion is extremely sonorous. The respiratory murmur is absent at left anteriorly, where nothing is heard but gurgling throughout. Behind, on the same side, nothing but gurgling and broncophony are heard at the upper part, whilst in the middle third the respiration is bronchial, and below it is vesicular. At right, under the clavicle, the respiration is rude, obscure, accompanied with gurgling; it is vesicular, and very feeble beneath the nipple, especially on a line with the above-mentioned prominence, less feeble behind in the corresponding part, and bronchial at the summit. The sound of the heart is feeble, not accompanied with impulsion; pulse 108, skin hot, head free.

The same condition of things was found on the following day, and subsequently; the patient had slight stupor, and died unexpectedly on the 21st, at one o'clock in the afternoon, having complained for some minutes previously of pain in the side, and sudden sense of suffocation.

Opening of the body twenty-two hours after death.—External condition.—Advanced stage of emaciation.

Head.—Hardly any infiltration under the arachnoid, pia mater very moderately injected and easily detached; cortical substance not deeply coloured; the medullary firm, not much moistened, and but slightly injected; three or four small spoonfuls of bloody serum in each of the lateral ventricles. The cerebellum, pons varolii and medulla oblongata, all natural.

Neck.—Larynx, epiglottis, and trachea, pale or of a livid green; their mucous membrane otherwise healthy.

Thorax.—Right lung free, except at the summit and posteriorly, large and heavy; its middle and upper lobes do not crepitate, and are hard throughout in places, except along the free border; the

inferior lobe crepitates almost every where; its cells are universally and unequally dilated, being from two to five times larger than common, and this dilatation is most marked at the base along the sharp edge, which is more rounded than common. The two middle and upper lobes contain a considerable number of crude tubercles, surrounded by a grayish tissue, not granulated; and at the summit of the upper one are found two or three small excavations, irregular at their circumference and lined by a very thin false membrane. The bronchial tubes of this lung are generally a little less transparent than natural, and some of them in the inferior lobe are a little dilated. The left lung is heavy, adheres throughout its whole extent to the costal pleura, is large, but less so than the right one, and is supple only behind and inferiorly. Its vesicles are no where dilated. At its summit there is a cavity of the size of a hen's egg, lined by a membrane of no great thickness, surrounded by a grayish tissue like that which has been described, which tissue is studded with tubercles, and constitutes almost the whole of the upper and a part of the lower lobe. A small tuberculous cavity was also found a little below the summit of the lower lobe; another about the size of a large nut was found near the base of the organ, behind, and was formed by a simple dilatation of the bronchial tubes; and at this point the dilated bronchium was precisely of the same aspect and thickness as that of which it was merely a continuation. Besides this bronchium, there were still seen two others in the same lobe, evidently dilated, and containing a yellowish opaque matter of the consistence of pus. With the exception of the bronchial tubes, which communicated with the upper cavity, and which were red, the others were pale, and like those of the right side, less transparent than natural. The pericardium contained a little bloody serum.

Abdomen.—Œsophagus pale, deprived of epithelium in its lower half; stomach three times its natural size, containing scarcely any thing but gas; its internal surface of a livid green, and no where injected; its mucous membrane mamelonated about two inches from the pylorus only, and over a very small surface, softened, and so much so in the large cul-de-sac as not to allow of strips being raised, whilst along the great curvature and upon both faces, strips of from four to five lines were obtained, and along the small curvature of from seven to eight lines, its thickness throughout being natural Small intestine distended with gas, triple its usual size, but resuming its ordinary dimensions after being opened; containing in its upper half a liquid of the colour of wine lees, which afterwards became brown and then yellow. Its mucous membrane was generally of the same colour as the matters with which it was in contact, and of natural thickness; softened and more adherent than common to the sub-mucous tissue in its upper half, where strips could not be raised; beyond this portion giving strips of from three to six lines, which became longer the nearer you approached the cœcum. In the course of the intestine, also, were in twelve ulcerations, some transverse, others longitudinal and

situated on the plaques of Peyer, about twelve lines long by from three to six broad, with prominent edges, the bottom being formed by the muscular tunic or by the thickened sub-mucous cellular tissue. Crude tubercles also existed opposite some of these points, under the peritoneal coat; softened tubercles existed in other parts, not ulcerated, and immediately beneath the mucous coat. The large intestine was also very much distended, and contained pultaceous matters, not moulded. Its mucous membrane was pale or of a livid red, not evidently thickened, and of a pulpy consistence; a single superficial ulceration was found in the middle of the transverse colon, about six lines in diameter. Liver rather large, pale, yellowish, fatty, of good consistence. Gall-bladder small, containing a small quantity of clear liquid. Spleen pultaceous, blackish, a little augmented in size. Kidneys of a livid red, easily penetrated; a small layer of cortical substance is raised when their enveloping membrane is torn off. Bladder small, and besides perfectly healthy.

In this patient, as in many of those whose history we are examining, the dyspnœa dated from infancy; it had increased during the last twelve years, and frequently within this period had assumed the form of paroxysms more or less painful at night; that is to say, it had followed a slow course, and presented all the characters of that form of oppressed breathing which occurs in emphysematous patients; and we may suppose that it was on this account that the attention of the observer was fixed upon a case of phthisis apparently of little interest. The prominence on the right side of the chest, the increased sonorousness, and on the contrary the diminished respiratory murmur in the same part, must have confirmed the data furnished by the dyspnœa; and finally anatomy placed its seal upon the diagnosis, which is worthy of note; not on account of the difficulties which it presented, but on account of the accurate application to a particular case of the data derived from experience. Similar cases must frequently occur, and be regarded as cases of simple phthisis. I observed one such ten years ago, at a time when emphysema but partially engaged my attention, and I then regarded, as an unsolved problem, a fact which now undoubtedly I could easily account for. I stated, indeed, in my researches in relation to phthisis, that in the ninth part of the tuberculous patients observed by me, the dyspnœa dated from infancy; that nevertheless this symptom could not be referred to the tubercles in these patients, many of whom were over fifty years of age; and I postponed to a future period the explanation of a fact, which further observation might easily, at least every thing seems to favour the supposition, have unraveled.

It may further be readily imagined that in cases analogous to that of which we are speaking, the symptoms of emphysema become every day less marked; that they finally disappear completely; and that on opening the body there are found scarcely any traces of the disease which was believed to have been both extensive and severe. We may imagine this change to take place through the development of the tuberculous affection, which might occupy, after a

certain length of time, all, or nearly all the parts which were primitively the seat of emphysema. This, which might easily have been foretold, I have recently observed in a woman aged thirty years. When admitted for the first time into the hospital, she presented, on the right side of the chest, the physical signs of tubercles not far advanced, and of well marked emphysema. A careful exploration, repeated several times during the first residence of the patient in my ward, always led to the same result. Having been somewhat relieved of the dyspnœa, which was extreme at the time of her admission into La Pitié, she went out after having remained some weeks. Soon after, she returned on account of the same symptom, but then every thing was changed. The extreme sonorousness of the right side of the chest had disappeared, and given place to a sound rather less clear than natural; the respiratory murmur was rude, and as it were bronchial, in some parts where it was scarcely heard before; whilst on the left side, anteriorly, there were found simultaneously in one and the same part, a well-marked prominence of the chest, an unnatural sonorousness, and a very feeble respiratory murmur. The patient in question again left the hospital after remaining there two months, and although the diagnosis was not verified by anatomy, it seems to me impossible to doubt its correctness. The emphysema must have been developed very rapidly on the left side, whilst on the right, where it was present to a remarkable degree at the time of the first admission of the patient into the hospital, it must have been masked by the progress of the tuberculous affection. Facts of this kind would not prove, as has been seen, either that the symptoms mentioned as characteristic of emphysema have not this value, or that emphysema itself can promptly disappear; and it is chiefly for this reason that I have thought proper to speak of the case in question.

It ought also to be remarked, in relation to the preceding case, that the emphysema was limited to the right lung, and that it was on the left side that the principal dilatation of the bronchia existed; so that this case is one of those which prove that the dilatation of the bronchia and that of the pulmonary vesicles are independent of one another.

Course of the Disease.—We have seen, when treating of the dyspnœa, that the course of the disease, although essentially chronic, was subject to great variations. In many patients it continued for a great number of years in a mild form, with but little change. In others it came on with some violence at the very first, whilst at the same time the patients thus attacked were not prevented from attaining a considerable age. It would also seem, from the example just quoted when treating of the fifth case, that the progress of emphysema may be pretty rapid in some patients. This proposition, however, is derived merely from one case, the history of which I collected in 1833, and which does not belong to the series which we have been analysing. This case is interesting on several accounts, and I deem it proper for this reason, and on account of the rarity, or at any rate the probable rarity, of similar ones, to give a detailed account of it here.

CASE VI.

The widow Marion, aged thirty-eight years, of a delicate constitution, but generally quite healthy, was admitted into the hospital of La Pitié, the 16th of November, 1832, on account of cephalalgia, which she had had for three years,—which had followed a violent moral emotion, and had afterwards continued almost uninterruptedly, so that the patient had not at any time been free from it eight days together. During the whole time her intelligence remained perfect, as well as the power of motion and organs of sense, only her appetite and strength diminished a little ; her menses appeared but eight times during the same period, and for the last time about eight months previous to her admission into the hospital. Leeches and blisters have been applied behind the ears, and venesection had been practised without any success.

The more difficult it was to discover the cause of the cephalalgia, the more carefully the patient should have been, and in fact was, examined at the period of her admission. At that time all the functions were found in the natural condition, the respiration good, the percussion of the chest quite sonorous, and no cough.

As no circumstance obliged us to make use of active and prompt medication, the patient merely took demulcents for six days, after which she was bled in the foot six ounces without relief; the pills of Miglen, taken three a day, were scarcely more successful. Finally, the 3d of December, some days after the discontinuance of these pills, a blister three inches in diameter was applied to the occiput, and by the following day the headache disappeared and never returned.

Up to the 6th of January nothing worthy of note; the patient was examined and interrogated every day, but complained of no uneasiness or pain in any part; eat three fourths of a portion, and remained in the hospital merely on account of some remains of feebleness which was not yet entirely dissipated.

But on the 6th, in the evening, slight hæmoptysis, which it was not thought proper to combat by bleeding. On the 7th, in the morning, a more considerable hæmoptysis (eight ounces of blood in two hours), subcrepitant râle accompanied with sonorous râle on the anterior part of the chest (v. s. ℥xii.) considerable relief followed the bleeding, and the spitting of blood did not return during the night.

On the 8th, in the morning, respiration slow, pulse 112, respiratory murmur and sonorousness of chest natural and similar at corresponding points ; no pain any where, tongue a little whitish, three ounces of liquid blood in the spitting vessel ; that drawn from the vein was covered with an orange coloured crust, three lines thick at its circumference ; (barley emulsion, gum potion, emulsion ℥vi., ten leeches to thighs). From this time to the 4th of February, the day of her death, the following circumstances were observed :—

The hæmoptysis recurred, but slightly, on the 14th and 15th of

January. On the 16th it had almost entirely disappeared, and did not again return.

The cough, which as late as the 13th still continued slight, became very inconvenient on the 26th, and continued so with some variations afterwards.

The dyspnœa, which had always been pretty severe from the time that the cough commenced, might be said to be excessive on the 30th, from which time to the day of her death the patient remained almost entirely upright in bed.

Percussion of the chest repeated several times from the 13th to the 30th, was found to be rather more sonorous beneath, above, and behind the right clavicle than the left. On the 13th, also, the respiratory murmur was purer under the left clavicle than under the right in the corresponding point, where, however, there was no distinct râle. But after the 26th a subcrepitant râle was constantly heard in this same region and behind on the same side, throughout its whole height; it did not exist at left behind till the 28th, and in front only after the 1st of February. A rude and somewhat bronchial respiratory sound was besides found at right posteriorly, above and below the root of the bronchia, for several days. The naturally feeble voice of the patient was almost extinct on the 1st of February.

The pulse, which was constantly very frequent, was 140 on the 31st, less frequent, by a few beats only, afterwards. There were copious sweats after the 1st of February.

Her appetite had not entirely gone on the 16th of January, and it rapidly grew worse subsequently.

On the 4th, at the period of the visit, the patient was in a state of stupor, and complained of not understanding what was said to her. A short time previous, at five o'clock in the morning, she told the sister that she had not passed so good a night for a long time, and by noon she was dead.

Sulphuric acid, lemonade, and a gum potion, with 15 grains of rhatany, were prescribed on the 15th of January, and agreed well with the patient. Eight ounces of blood were taken by venesection on the 28th, which was covered by a crust two lines thick at its circumference. A half ounce of syrup of white poppies was daily mixed with the potion after the 12th, and three grains of kermes were added to it after the 1st of February.

Opening of the body twenty-one hours after death. External condition.—Second stage of emaciation; no lividity; slight rigidity.

Head.—Nothing remarkable exterior to the dura mater; a small quantity of black blood in the cerebral veins posteriorly; very slight infiltration under the arachnoid at the same part; arachnoid otherwise healthy; pia mater very little injected, and only behind; a small spoonful of serum in each lateral ventricle; the whole substance of the brain very little injected, of natural colour and firmness.

Neck.—Pharynx, œsophagus, epiglottis, larynx, and trachea, in a natural state.

Chest.—Lungs perfectly free, except some adhesions at the right summit: both of them large, and not collapsed by the opening of the chest. Right lung twice as heavy as natural, partly on account of a slight engorgement of the inferior lobe (sanguine infiltration), partly owing to other lesions, which will be described in order. Its cells were every where dilated, and three, four, or five times larger in diameter than natural; that of a considerable number of them near the sharp edge being from one to two lines, so as to form, when opened, small excavations, not lined by false membranes. Both tubercles and demi-transparent gray granulations also existed throughout the whole height of the organ, diminishing in size and number from the summit to the base; and at one inch and a half from the top were found two small excavations, of two lines and a half in diameter, filled with pus and lined by a thin and soft false membrane. The bronchial tubes contained a pretty large quantity of thick mucus, and were all of them evidently dilated. Those of the upper lobe were less so than the others, without any appreciable alteration in their colour or thickness; those of the middle lobe were a little thickened; those of the lower lobe were both thickened and red, and many of them contained transverse folds, valvular as it were, formed by the thickened mucous membrane, and at the same time were from three to five times larger than natural. The left lung was a little less heavy than the right, but otherwise presented the same lesions, with the exception of the tuberculous excavation, which did not exist in it, and the dilatation of the cells which was also a little less considerable than on the right side. The pericardium contained a spoonful of limpid serum; the heart was of moderate volume and firmness.

Abdomen.—The stomach was of medium size, looked as if it were strangulated in the middle, and contained a small quantity of greenish and very unctuous liquid. Its mucous membrane presented an arborisation of an inch in extent near the pylorus, was pale every where else, of good consistence and thickness throughout, and not mamelonated. The small intestine was of the usual size, contained a yellowish matter in its first half, whitish in the remainder, under which was found a pretty large quantity of mucus. Its mucous membrane was of natural thickness and consistence. (Strips of from six to eight lines throughout its whole extent.) The glands of Brunner numerous. No ulceration or tubercles any where. The sub-mucous tissue of the ileum was slightly injected. The large intestine was not at all remarkable as regards its size; contained pultaceous matters in its first portion, and moulded ones afterwards; its mucous membrane was injected in a good many places, not thickened, and gave throughout strips of only two or three lines; numerous crypts, confluent at intervals, existed in the first half of the organ. The liver was higher by an inch and a half than it was broad, extended as far as the fifth rib, without any appreciable alteration. The gall-bladder contained a pretty large quantity of a dark-coloured liquid. Nothing else remarkable.

Thus, after a month's cough, with bloody sputa, fever, rapidly

increasing oppression, and a pretty severe hæmoptysis at the commencement, the patient dies; and we find, upon opening the body, a general emphysema of the lungs, of very marked character, rather more so at the right than at the left summit, accompanied with general dilatation of the bronchia, and with tubercles, both the number and size of which went on diminishing from the summit to the base of the organ. Now, as it is impossible to refer the origin of the dyspnœa, or of the tubercles, or of the dilatation of the bronchia, or that of the pulmonary vesicles, to a period more remote than that of the first appearance of the hæmoptysis, we must admit that this short space of time was sufficient to allow the advance of the triple disorder which we have noticed, to the stage in which it was found. We cannot date the lesion of the lung beyond the period mentioned, both because the condition of the chest was carefully determined at the time of the patient's admission, and nothing anormal found there, and because the same result was arrived at after a similar examination, repeated with still greater precision two days subsequent to the occurrence of the first hæmoptysis.

The rapid course of the dilatation of the bronchial tubes, of the pulmonary vesicles, and of the tuberculous development, being well established, we should observe that, with the exception of these changes, and of the softening of the mucous membrane of the colon, the other organs were healthy; that these lesions would not be sufficient to account for the death of the patient, if they had not been suddenly induced; and that hence this fact is a new proof, although an indirect one, of the extremely rapid course of the disease in the present instance.

Further, it is extremely probable that, when once the attention of physicians is directed to the study of emphysema, they will not fail, at least occasionally, to meet with cases of the kind above mentioned.

ARTICLE FIFTH.

CAUSES OF EMPHYSEMA.

The history of the cases which I have observed is very incomplete, with respect to the causes of the affection. The same is true of those whose history Jackson has recorded, except in one point of view, which will be mentioned at the conclusion of this article. Most commonly, we neglected to enquire into the circumstances which either coincided with, or for a certain length of time preceded, the commencement of the dyspnœa; so that, with reference to the present question, the analysis of our observations can be brought to bear only on a few points.

With the exception of those cases in which the dyspnœa dated from early youth, and in which the cough did not commonly supervene till much later in life; with the exception of these cases, I say, the oppression was very far from being always preceded by pulmonary catarrh, as has been previously stated; and in several patients this catarrh did not occur for several years after the com-

mencement of the oppression. Hence the inevitable conclusion is, that emphysema may, and very frequently does, in fact, originate independently of pulmonary catarrh. This conclusion is also somewhat confirmed by the fact, that pretty frequently the dyspnœa appears not to have augmented appreciably, in consequence of an attack of severe acute pulmonary catarrh. And if we recollect that the maximum of the emphysema is situated along the free border of the lungs and its neighbourhood, whilst acute severe pulmonary catarrh, on the contrary, is seated at the posterior-inferior part, we must conclude that if this catarrh has any influence upon the development of emphysema, this influence must be slight, and but rarely brought into play. On the other hand, pneumonia, notwithstanding the dilatation of the vesicles by which it is accompanied, seems also to have nothing to do with the production of emphysema (page 511); so that two of the affections which one would have supposed, at first sight, most likely to cause emphysema, either contribute very little, or not at all, so far as we could discover, to its production.

Nevertheless, Laennec regarded dry pulmonary catarrh as the cause of the dilatation of the pulmonary vesicles, and conceived that the latter resulted from the presence of a viscid mucus in these organs, which, as they could not easily relieve themselves from it, became necessarily dilated. But this explanation, which at first seems very natural, does not accord with the facts which we have already stated, and is in opposition also to the following one—viz. that whatever may be the size of the dilated pulmonary vesicles, even if they be as large as a cherry-stone, we find them empty and devoid of mucus or false membrane. In fact, putting aside the explanation of Laennec—and it seems to me that we must do so—we cannot account for the dilatation of the pulmonary vesicles; but what does it signify, provided the above facts be correct? Do we understand any better the dilatation of the bronchial tubes? Here, surely, we cannot attribute the dilatation to the prolonged presence of mucus in the dilated organ caused by an obstacle in its passage; consequently we must admit a different cause from that which presides over the morbid dilatation of most of our organs. This proposition cannot be questioned with reference to the bronchial tubes, since there are instances, as in Case 6, where the mucous membrane of the bronchia—instead of being tense, as it should be, if we suppose the dilatation mechanical—presents folds of a valvular form. So that here, as well as in emphysema, we must admit, at least in a great number of cases, a force analogous to that which presides over the extension of hollow organs, and in virtue of which these latter enlarge without our being able to account for it by means of any obstacle, or mechanical cause.

In two cases where the dyspnœa had existed—in the one for five, and in the other for thirty years—at the time when the patients were admitted into the hospital, it came on immediately after a violent moral emotion, and never afterwards disappeared. Was this

merely a coincidence? On the contrary, had the moral emotion some part in the production of the disease?

The question of its hereditary nature was studied by Jackson. In order to arrive at conclusive results in this respect, he made particular enquiries of a considerable number of patients in reference to the habitual state of health of their fathers and mothers, sisters or brothers; in reference to the disease of which they died, as also their age at the time of death. His questions were principally directed to the state of the respiration; to the duration of the difficulty of breathing, when it existed; to the condition of the limbs, their volume, &c. He regarded as established only those facts which were attested by intelligent patients, whose memory was good, and who uniformly gave the same replies to the same questions. Proceeding with this reserve, which is demanded by the nature of the subject, he arrived at the following results.

1. Of twenty-eight patients affected with pulmonary emphysema, eighteen were the offspring of parents who had been, either father or mother, attacked with the same affection, and several of whom had died in the course of it. In some cases, the same was true of the brothers and sisters.

2. Of fifty individuals not affected with emphysema, three only were descended of parents who laboured under this disease; whence it follows, every thing being otherwise equal on both sides, that emphysema is frequently an hereditary affection.

Another not less remarkable fact, for which we are also indebted to Jackson, is, that hereditary influence is much more marked in cases where emphysema dates from early infancy, than in those in which it commences just before or subsequent to the age of twenty years. At least this proposition seems to be rigorously derived from the following statement—viz., that of fourteen individuals whose dyspnœa was traced to early youth, fourteen had asthmatic parents; whilst of fourteen patients attacked later in life with emphysema, two only were the offspring of parents who had died of the same disease.

ARTICLE SIXTH.

FREQUENCY OF EMPHYSEMA.

Physicians having hitherto paid but little attention to emphysema, notwithstanding the labours of the illustrious Laennec, it will perhaps be asked—notwithstanding the numerous details into which I have gone in reference to the diagnosis, and the care which I have taken to show the relation which exists between the symptoms and the lesions from which they arise—it will be asked, I say, whether I may not have deceived myself, and whether emphysema is in fact as frequent as the preceding observations would induce us to believe. The greater part of the cases analysed in this memoir were in fact collected during the space of twenty months; and I have said above, that in a still shorter space of time—eight

to ten months—Jackson had observed forty-one patients affected with emphysema. But so far from admitting here an error by which the number has been exaggerated, I am convinced that Jackson did not recognise every case of emphysema which existed in the wards in which he conducted his observations; because he could not find sufficient time, notwithstanding all that which he devoted to observation, to enable him to study all the patients placed in my division, in such a way as to allow of his recognising emphysema of every degree in those who were attacked with it. Besides, the reader has not forgotten that, of fifty patients carried off by the cholera, and where the autopsy was made with great care, twenty-three were affected with emphysema at various degrees; and, as I have not chosen cases, we must conclude that emphysema is a frequent disease, at least in individuals who die at the age of those of whom we have been speaking, and which has been previously mentioned.

To be sure it will perhaps be said, on account of the dyspnœa which accompanies cholera, when it is severe, that this disease may have had some influence upon the production of the affection of which we are treating. But without discussing the reasons which would lead us to attribute the oppression of cholera patients to a cause altogether different from dilatation of the pulmonary vesicles, I will remark, that a dilatation so recent as that which we must here suppose, the cause of which could not have operated, in many cases, longer than a few hours before death, would very probably have disappeared in a few hours after the fatal termination; and that this dilatation followed the same laws, affected the same preference for certain parts of the lungs, as in those individuals who were carried off by other diseases, being more marked near the free border than elsewhere, and more frequent in old than in young persons. So that we cannot have the least doubt that the two affections, cholera and emphysema, were independent of one another.

ARTICLE SEVENTH.

OF THE TREATMENT OF EMPHYSEMA.

Doubtless it would, above all things, be most important to the reader, to find here laid down the whole of the means by which emphysema may be prevented, or, in other words, the prophylactic treatment of the disease. But, as we have seen above by an examination of the cases analysed, the causes of emphysema are nearly unknown to us. We merely know that it is often hereditary. Nothing can be said, then, positively upon this subject; and all we can do in a memoir like the present, where we have endeavoured to advance nothing which could not be rigorously deduced from the facts, is to state that it is proper, with a view to preserve ourselves from emphysema, to avoid the most general and best

known causes of disease, and of those of the lungs in particular, and also every thing which, according to the results of experience, may bring on, when emphysema already exists, an attack of dyspnœa—as exposure to dust, to an atmosphere more or less charged with deleterious substances, to moisture, &c., and, in general, to every thing which is capable of augmenting the symptoms of emphysema.

When once developed, it is necessary, with reference to the treatment, to distinguish the cases where emphysema is simple, from those in which it is complicated; those cases in which the patient experiences paroxysms of dyspnœa, from those in which paroxysms do not exist.

If emphysema is simple, mild, that is, accompanied by but little oppression, by slight pulmonary catarrh, with scanty and easy expectoration, without paroxysms of dyspnœa, it is proper—as results from the analysis of the facts previously detailed—to avoid violent emotions, exposure to dust, which I have several times known to bring on paroxysms of oppression, moisture, especially exposure to fogs, which is so often followed by coryza, and afterwards by pulmonary catarrh, or an increase of that which already existed, and consequent paroxysms of dyspnœa. The patient should avoid every thing which quickens the respiration and increases the necessity of breathing, fatigue of body and mind, violent and repeated emotion of all sorts, and too animated conversations. It would be well also to endeavour to find out whether change of residence, going from one city to another, would diminish the dyspnœa, since it is said that a considerable number of asthmatic patients (doubtless these persons were for the most part affected with emphysema) under such circumstances, have recovered more or less completely from their difficulty of breathing, whilst the dyspnœa resumed its primitive violence after they returned to their original places of residence.

As for the rest, I have never observed, whatever might be the severity of the pulmonary catarrh which accompanied the emphysema, that medicines called incisive, such as polygala, simple oxymel, oxymel of squills, amygdaline soap of soda, &c., so much recommended by Laennec, diminished, as he assures us, the dyspnœa and favoured expectoration. I do not doubt that Laennec attributed to the polygala, the oxymel, &c., that melioration which, in patients belonging to the working classes, is the result of repose in bed, of diet and the use of diluent drinks, as soon as they are admitted into the hospital—a melioration which is due, in fact, to all these means. That which I have just said of the polygala, applies equally to bleeding, in the cases above mentioned. One medicine only, and Laennec has mentioned it, had a happy influence upon the dyspnœa of the greater part of the patients attacked with emphysema of the lungs, whose history I have recorded; I mean opium in all its forms. Almost all those to whom I gave it were remarkably relieved, (twenty-six out of thirty of those in whose history I have carefully noted the phenomena which fol-

lowed the administration of this medicine,) and the symptoms resumed their pristine violence as soon as its use was suspended, unless they had been already quieted for a certain length of time.

But it is especially in severe cases, where the paroxysms of dyspnœa are very great, so that the patients seem threatened with approaching suffocation, that opium is useful, even if at the same time the presence of a very extensive subcrepitating râle seems to indicate that these paroxysms depend upon a violent acute pulmonary catarrh. Four times, indeed, I have been able to convince myself of it, in patients who were not over thirty-eight years of age, and who presented symptoms most readily recognised, of an almost universal emphysema, which was at the same time uncomplicated and far advanced. They were in a state of inexpressible distress when I saw them for the first time, breathing more than fifty times in a minute, the pulse very frequent, (120 to 140 pulsations in a minute,) subcrepitant râle at the posterior part of the chest, in its two lower thirds or three fourths. One of them was bled three times in two days without the least benefit, the dyspnœa remaining the same, and he appearing to be in a more critical situation after the third bleeding than before the first. I then had recourse to the gummy extract of opium to the amount of two grains in a few hours, and the next day the dyspnœa was very moderate, the respiration much less frequent, and the patient so much relieved that he believed himself cured. In the two others, no blood was drawn; opium was given almost immediately after their admission into the hospital, and the relief was not less prompt.

If the paroxysm of dyspnœa should be owing to an atmosphere charged with dust, it would be necessary, in the first place, to remove the patient from it, and place him in an open situation where there was a free circulation of pure air; and then, as may readily be supposed, whatever may be the severity of the dyspnœa, sanguine emission would be useless, and, in accordance with what has been previously stated, some opiate preparations should be given, without, at the same time, depriving ourselves of the use of some revulsive or diffusible excitants, the utility of which, however, I would not undertake to answer for.

Of all the complications which may arise in the course of emphysema, the most remarkable, assuredly, on account of the influence which this disease exerts upon its development, is hypertrophy with dilatation of the cavities of the heart. This complication undoubtedly demands some modifications in the treatment of the patient; but we must here never lose sight of the fact, that the dyspnœa which he experiences, is in a great measure owing to the emphysema—that is to say, to an affection which is not sensibly influenced by blood-letting; and then, as we cannot expect any great relief through its means, we must, for that very reason, make use of it sparingly.

Experience having taught me nothing positive in relation to the operation of other means which might be tried in the treatment of

emphysema, I shall conclude what I had to say upon this point, by reminding the reader that those who are affected with this disease in an uncomplicated form, are free of fever; that their appetite is commonly good, and that it ought to be satisfied, taking care only to interdict the use of heating meats, and of every thing which, by accelerating the circulation, might and would augment the habitual dyspnœa.

ERRATA.

At page 514, 14th line from the bottom, for *In those who*, read *In some of those who*, and at the 11th line from the bottom, for *in the eighth part of the cases*, read *in the eighth case.*

Note.—At the commencement of the paragraph in which the above errata are found, there is, most probably, an error in the French copy. The paragraph begins thus, "*In those who died*, the form of the chest was attended to," &c. It is clear that a portion only of those who died is here intended, and we may conclude, from what follows, that this portion includes eight. It would then read thus, "*In eight of those who died*," &c.

OBSERVATIONS AND EXPERIMENTS

ON THE

LUNGS OF NEW-BORN CHILDREN,

IN RELATION TO

MEDICAL JURISPRUDENCE.

BY ALFRED S. TAYLOR.[1]

There are few subjects which demand a greater share of attention, on the part of a medical jurist, than the application of the pulmonary tests in a case of alleged child-murder. Much has already been written and published on the medico-legal relations of infanticide; but the subject is far from being exhausted. Indeed, the great difference of opinion which prevails among many estimable and accomplished men in the profession, relative to the utility of the hydrostatic test, is a sufficient inducement, to those who are inclined to take the affirmative side of this question, to collect all the cases which may present themselves to their notice, and endeavour to show in what instances the test may be safely applied, and in what its use should be avoided. Through the kindness of my friend, Dr. Ashwell, I have lately had the opportunity of examining the bodies of four new-born children. Two of these cases are somewhat remarkable; but as the facts connected with the birth of each child were known, it is presumed that the two others will also be found in some respects worthy of attention. I shall describe the examination of the subjects in the order in which it appears to me it ought to be conducted, in every investigation relative to child-murder.

CASE I.

What was the age or degree of maturity of the child?

This child was of the male sex. Its body, measured from the vertex to the sole of the foot, was eighteen inches long; and the point at which the umbilical cord was inserted, was just about the centre of its length; the body weighed 33465 grains—*i. e.* nearly

[1] From Guy's Hospital Reports, for October, 1837, p. 318.

six pounds. The hair on the head was abundant, and about an inch in length. The bones of the cranium were firm, and did not readily overlap each other on compression. The nails were well developed, and reached fully to the extremities of the fingers. The testes had passed into the scrotum. The membranæ pupillares had disappeared. The skin was pale and smooth, presenting no sign of desquamation. Around the neck and shoulders there was a diffused redness. From the whole of these data, we are entitled to draw the inference, that this child was either mature, or very near maturity.

Had the child lived to respire?

The body was full and plump. The chest was narrow, and somewhat flattened anteriorly; on opening it, the cavity was found to be somewhat small and contracted. The thymus gland was large, and of a livid colour. The heart and pericardium were prominent. The lungs were placed so far back in the cavity, as to be scarcely visible; the anterior edges of these organs, which were sharp, alone projected forwards; but they did not reach or overlap the sides of the pericardium. They were of the usual livid or blue colour observed in children which have died without breathing. On firm compression, in every part of their substance, there was not the least sign of crepitation.

As I considered this a very favourable case for the experiment, I resolved to inflate the lungs before removing them. While the cavity of the chest was thus exposed, a bent glass tube was introduced into the mouth, the nostrils were closed, and several moderately-violent efforts were made to inflate the organs, but at first without effect. After five or six strong inflations, bright-coloured insulated patches, almost of a vermilion redness, began to show themselves; chiefly in the *right* lung, which appeared to receive air much more readily than the left. The anterior and inferior portions of the lower lobe, with the anterior margin throughout, were the parts which became inflated. The left presented similar patches, principally upon the external surface and lower portion. After each inflation, the lungs slightly collapsed; and although five or six long-continued inflations were made, at least four fifths of the organs remained undistended. At this time, they projected but very little forwards, becoming only somewhat more closely applied to the posterior and lateral parts of the pericardium, but by no means concealing it.

Before removing the lungs from the chest, it was observed that the ductus arteriosus was of equal diameter, from its origin to its junction with the aorta. Its calibre was such as is usually found in a child which has not breathed. The heart contained a quantity of grumous blood. The foramen ovale had not either changed its position, or become contracted in its dimensions. The axis of the crescentic edge of the membrane, bounding it inferiorly, was directed upwards, and to the right.

Ligatures having been applied to the pulmonary arteries and

veins, which, it may be observed, were distended with blood, the lungs were removed from the chest. They were large, and presented, externally, the variegated colour already described; but they had obviously not become inflated posteriorly. In the light-coloured portions, they were feebly crepitating beneath the fingers. They weighed 586 grains, which is about the weight of lungs that have not respired.[1]

By the test of Ploucquet—*i. e.* dividing the weight of the body by the weight of the lungs—the ratio obtained was as follows:—33465 : 586 = 57 : 1. This falls within the ratio derived from the lungs of children that have not breathed.

We have now then collected, I think, sufficient data to answer the question proposed. The colour, volume, and consistency of the lungs, their absolute weight, and the ratio obtained by Ploucquet's test, render it in the highest degree probable, if not absolutely certain, that this child *had not lived to respire.* It will be here remarked, that one strongly corroborative sign of respiration connected with the lungs, namely, their *specific gravity*, was wanting. No inference could be derived from the employment of the hydrostatic test in this case, since the lungs had been artificially inflated; but it cannot be doubted, on attending to the whole of the circumstances, that, had not air been thus introduced into the organs, they would have sunk in water, either entire, or divided into pieces.

I shall next state the nature and results of the experiments performed on the lungs. When separated from the heart and thymus gland, and placed on temperate distilled water, they readily floated. Their specific gravity was found to be .943. The organs were not in the least putrefied; indeed, there was no sign of putrefaction in the body, the child not having been dead above twenty-four hours, and the temperature of the atmosphere being very low. To determine the nature of the changes which they were likely to undergo, the lungs were again placed in the chest, and the body was set aside for a week. At the end of this time, although there was no appearance of decomposition, the colour of the lungs had become much altered on the surface. The parts to which the atmospheric air had had slight access, were of a pink red; the other parts were of a slate gray. The bright red patches, which had resulted from inflation, had entirely disappeared; and the inflated could no longer be distinguished from the uninflated portions. There was now not even the slightest feeling of crepitation, on pressure, in any part of the pulmonary structure. Placed

[1] The air, which had been artificially introduced into them, could not of course add to their original absolute weight, since they were weighed in air. This is one of the diagnostic marks of artificial inflation.

According to Orfila (*Méd. Lég.* I. 350.) the fœtal lungs *always* weigh less after artificial inflation than before. In one or two cases only I have observed a very slight difference; which appeared to me to depend upon the air, when violently introduced, forcing out a small portion of the blood contained in the vessels.

on distilled water, they floated as well when united as when separated from each other. There was no perceptible difference in the degree of buoyancy of the two lungs.

Each lung was then separately compressed, *entire*, in a folded cloth, as well as between the fingers; but the most violent pressure, for some time continued, did not so expel the air, as to cause either of them to sink when again placed on water.

The right lung was next divided into twelve pieces. In cutting it, there was no obvious crepitation; but the parts containing air were distinguished by their bright crimson redness, the surrounding parenchyma being of a deep Modena-red colour. On examining the divided portions, it was observed that they were perfectly free from disease. Each of the divided portions which floated, was separately subjected to firm pressure in a folded cloth, as well as between the fingers; care being taken not to carry the compression to such a degree as to destroy the organic texture of the lung. After this, they readily sank, when placed on water.

The left lung was now inflated to the greatest possible extent, by the introduction of a blow-pipe into the bronchus. It became universally of a light *pink-red colour*, not distinguishable from that of lungs which had respired; and it was strongly crepitating under the finger.[1] On examining it externally, there was no air to be seen extravasated, or collected in vesicles beneath the surface —an appearance not unfrequent in artificially-inflated lungs. When placed on water, this lung was, of course, extremely buoyant. It was again compressed entire; but the air could not be expelled from it, so as to make any perceptible difference in its buoyancy. It was then divided into twelve nearly equal portions; there was a distinct crepitus perceived in the division; the cut surfaces had a mottled appearance, and, on compression, air was freely extricated. The divided portions were separately compressed in folded cloth, aided by the fingers; and each piece experimented on, successively lost its air, and sank. Several of them required a frequent and violent repetition of the pressure, before the air was sufficiently expelled from their substance to render them heavier than water. Care was taken so to regulate the degree of compression, as that the structure of the lung should not be destroyed. It is proper to mention, that not one of the divided portions presented, on examination, any appearance of disease.

These experiments, which, I may observe, I have for some years past frequently performed upon the lungs of still-born children with the like results, appear to me to establish, that air, introduced by artificial inflation, may, under all circumstances, be expelled by compression, if the experiment be properly performed, and the

[1] It is singular, that so acute an observer as Professor Meade should deny that the colour of inflated lungs resembled the lungs which have respired. The inflated organs he describes as having a grayish-white or a dusky-red colour (*schmutzigröthlich*).—*Handbuch*, 390.

pressure continued a sufficient length of time.[1] The very violent compression, described as necessary for the expulsion of air from the divided portions of the left lung, in this case, is not likely to be often required in practice; since it is doubtful whether a lung could become so fully inflated as this was, while lying within the cavity of the chest. Besides, it must be remembered, in alleged infanticide, inflation can only be resorted to for the purpose of *resuscitating* a child. Violence in such a case is therefore out of the question; or if we found, by the appearance of the lungs, that very great violence had been used, there might be some ground for suspecting that the intentions of the party who performed the experiment could not have been quite so innocent as they may be represented.[2]

It will perhaps be deemed not inappropriate, to mention, in this place, the possible effects of violent inflation on a living child. M. Leroy d'Etiolles showed, so long back as 1826, that when air was forcibly propelled, by inflation, into the lungs of living animals, death was a very speedy consequence. The animals upon which he experimented, were rabbits, middling-sized dogs, and sheep. Out of seven sheep, four died immediately, and three lived a

[1] The compression of inflated lungs, as a diagnostic sign, was, I believe, originally proposed, many years since, by the ingenious Béclard. The late Mr. Jennings (of Leamington) has the reputation of having first employed it, and shown its value in this country. When individuals, pursuing the same course of experiments, arrive at similar conclusions, it is, I conceive, a matter of satisfaction to both; since their results serve to afford a mutual corroboration of each other's views. It was thus that the late Mr. Jennings and myself were situated. We were, it appears, without any knowledge of, or communication with, each other, endeavouring to remove some of the difficulties connected with the subject of infanticide; and we have formed very similar opinions relative to the effects of compression on artificially-inflated lungs. The real facts of the case are, if I mistake not, these. Mr. Jennings' observations were published in the volume of the Transactions of the Prov. Med. and Surg. Association for the year 1833. My Essays were in the hands of the editors of the London Medical and Physical Journal, in the autumn of 1832; and they were published in the numbers of that journal for November, 1832, and January and May, 1833. It is not improbable that both Mr. Jennings and myself may have been anticipated in our researches by others in this country; at any rate, it is certain that similar experiments were made by the German medical jurists upwards of sixteen years ago.—*Vide* Meade's Ausführliches, Handbuch der G. M. Dritter Theil, p. 493.

[2] We must, of course, make all due allowance for inexperience on the part of the person who performs the experiment. Unless the child's nostrils be closed, it is scarcely possible, without very long-continued efforts, fully to inflate the lungs. Loder, in an experiment of this kind carefully conducted, found, to his astonishment, on an inspection of the body, that the lungs were almost universally of a fœtal colour, with only a few lighter patches scattered about them. When placed on water, they instantly sank. When divided and again tried, a few pieces, amounting only to about *one fourteenth part* of the lungs, floated.—*Metzger, Op. cit.* 391. *Meade, Op. cit.* 389. The experiments of M. Billard have established, that the more immature the child, the more difficult it is to inflate its lungs throughout their whole mass.—*Orfila, Op. cit.* I. 362. Martini mentions the violently blowing of air into the mouth of a new-born child as one of the methods of committing infanticide.—*Introduzione alla Medicina Legale*, III. 17.

quarter of an hour. On inspecting the bodies, the air-cells were not found lacerated, at least in those animals in which death was immediate; but there was evident extravasation of air in the chests of the others.[2] Experiments like these, performed on animals which are breathing, are not perhaps strictly analogous to those instituted on new-born children that have not respired; but the analogy may be permitted to hold, in so far as to allow of the inference, that violent inflation may effectually destroy the life of a new-born child, whether it have manifested life by breathing or not. It is scarcely possible to imagine, that a person who had sufficient knowledge to attempt inflation for the purpose of resuscitating a child, would not, at the same time, have sufficient judgment to avoid great violence in its performance. Hence it follows, that, for all the purposes of practice, the experiment of compression may be expected to succeed in forcing the air out of lungs that have been artificially inflated.

When, besides the effect of compression on the lungs of this child, we take into consideration their other characters—as, for example, their absolute weight, and the relation of this to the weight of the body—we have strong grounds for asserting, supposing that the whole history of this case had been unknown, and the lungs had actually been found to contain air on opening the thorax, that the child had not lived to respire. The next question to be solved, is:

Was the child born alive?

I need hardly observe, that, although we have shown that a child has not breathed, this is no proof of its not having been born alive. Children may be born, and live after birth, without respiring, certainly a sufficient time to be murdered; and the law of England, at least as it is interpreted by some of the high authorities who at present occupy the Judicial Bench, does not insist upon proof of respiration as a necessary mark of live birth—a principle, in the justice of which we must all agree; although it may be extremely difficult to produce evidence of life after birth, where respiration has not been performed; since most of the physical proofs of life, met with in the new-born child, are dependent on the establishment of this process.

Among the evidences of live birth are the changes in the divided portion of the umbilical cord, which have been so accurately described by Billard. These, which will be treated of hereafter, are considered, by M. Devergie and others, to be so well established in the order and time of their appearance, as to furnish proofs, not only of a child having been born alive, but of the actual period, within certain limits, which it has survived. It is to be observed, however, that where we do not find signs of respiration in the

[2] *Vide* Orfila, Médecine Légale, I. 360. Also, Arcana of Science, 1828, p. 100.

lungs, we are not certain of meeting with these changes in the umbilical cord; and where proofs of respiration exist, they are but of secondary importance. The case of this child offers a remarkable illustration of the fallacy of exclusively trusting to their presence, as evidence of the period of survivorship after birth. The abdominal portion of the umbilical cord presented all the characters which I have frequently witnessed in children that had survived their births some hours. The skin, covering it for about half an inch from its abdominal insertion, was smooth, and perfectly continuous with that of the abdomen. Beyond this, and as far as the ligature, *i. e.* for about an inch in length, it was corrugated, and of a yellow colour, but not dry or transparent. At the junction of the sound with the shriveled portion of skin, there was a *faint line of redness*, like that of incipient inflammation, gradually diffusing itself towards the skin of the abdomen.[1] This child was born dead; but it so happened, that there was an opportunity of comparing the appearances of the cord with those of another child which had survived its birth twenty-four hours. The only observable difference was, that in the latter the cord was dry and transparent. The examination of both children had been made about twenty-four hours after death. The appearances of the cord, as will be stated presently, may sometimes show that a child has lived after birth, although not the period which it has survived.

I have already anticipated the conclusion, to which we should have come, respecting this child—that there was no evidence of its having been born alive. The absence of all signs of respiration is, *cæteris paribus*, a good presumption against live birth; for few children come into the world alive without respiring, either during birth, or very soon afterwards. But there were certain appearances about this child's body, which proved that it must have been living at or about the time of its birth. The left side of the head and neck, as well as the left shoulder, were deeply reddened. The redness was clear and diffused, not livid, or defined in patches. On cutting into the skin at these parts, the colour was found to depend on a slight infiltration of blood in the upper layers; but there was no decided effusion. The presence of this discoloration was a clear proof, either that the blood was circulating, or that it had but very recently ceased to circulate, at the time

[1] This is not a singular instance. M. Devergie met with a case, in which the umbilicus of a child, that had been born dead, presented a similar appearance. The skin around the navel was marked by an inflammatory redness.—*Méd. Lég.*, I. 507. It is the more important to notice the occurrence of this condition of the cord in a still-born child; since many have regarded the redness as the best means of distinguishing the separation of the cord, during life, from that which takes place after death. Not only, however, may the redness be absent in the vital separation; but it may be present in the child which has come into the world dead. Billard supposed that those cords which were small in diameter alone separated, without an inflammatory redness ensuing. This observation requires to be confirmed by the researches of others.

of its production. No pressure or force, applied to a child which had been dead some hours, would have sufficed to produce this appearance. I was informed that the child was alive during parturition; and that the labour had been difficult and tedious. The superficial discoloration about the head and shoulders would have led to this inference, supposing that the facts had not been known. There was no particular appearance in the body, to account for death; but it is highly probable that the child had died from exhaustion, during the protracted delivery. It is possible, also, that the circulation between it and the mother had been interrupted before the act of respiration could be performed.

This case bears out the general correctness of the data, upon which medical jurists are accustomed to act in investigations respecting infanticide. Some have laid down the doctrine that the pulmonary vessels are collapsed, and destitute of blood, in a child which has not breathed. This doctrine can scarcely have been founded on actual observation; since, as in the case before us, although a child has not breathed, and has been born dead, they are often distended with blood. The quantity contained in the vessels may vary materially, before and after complete respiration; but this difference is only relative, and it is therefore inapplicable as evidence, either for one condition or the other.[1]

The lungs weighed 586 grains. This is stated to be about the average weight of lungs which have not breathed. Observers, of course, differ materially in their statements on this point, but all make a considerable difference between the weight of respired and the weight of non-respired lungs. The mean weight of healthy lungs, calculated from their weight in six mature children which had died without breathing, I found to be 569 grains; while the weight of well-developed organs, which have fully respired, is rarely under 1000 grains. In one instance of a still-born child, the lungs weighed no more than 300 grains; but this child had evidently not reached maturity. On another occasion, a case of suspected child-murder, in which respiration was highly probable, although not certainly ascertained, the lungs weighed 1950 grains. I remarked, in this case, what I believe to be a general fact, that the right lung was much heavier than the left. The right lung weighed 1110 grains; the left, 840 grains. The cause of this increase in the absolute weight of the lungs after respiration is well known to be due to the increased circulation of blood through the minute vessels of the organs. In order that it should be properly observed, it is necessary that the process should have become well established. Respiration, therefore, differs from all other causes which render the lungs buoyant in water, in the fact that this process increases their absolute weight, while it diminishes their specific gravity. Of all the physical changes in the lungs, as evidence of respiration, the least liable to fallacy is that connected with the increase of weight. The lungs may be of a light red colour, fully

[1] See Orfila, Méd. Lég. I. 341, 363.

prominent in the chest, highly crepitant to the feel, and very buoyant in water; yet, if their absolute weight be not raised above that of the fœtal condition, it is almost, if not absolutely, certain that respiration has not been performed. The other appearances may be due to various causes; but supposing the lungs to be healthy, no other cause can add to their weight but the change in the pulmonary circulation, dependent on the introduction of air into their substance by respiration.

Ploucquet's test gave a ratio of the lungs to the body, of 1 : 57. The founder of the test conceived that the average ratio for children which had not breathed would be 1 : 70; and for those which had breathed, 2 : 70 or 1 : 35. Subsequent researches, however, have shown that these numbers must not be considered to represent the true averages.[1] The most serious objection to the employment of this test, in cases of infanticide, is, that the lungs and the body are liable to vary in their relative weights, in children of the same age; and, *à fortiori*, this variation must exist to a great extent among children which have reached different ages. It is easy to understand, that, during gestation, there may be various degrees of development in the body, without any necessity existing for a corresponding development taking place in the lungs. It is unnecessary to enter into speculations relative to the causes: experience has shown that such variations really exist; and all that the medical jurist has to consider, is, whether the differences can be reduced within limits which may make the test available in practice. M. Devergie has found that Ploucquet's test affords no satisfactory results, as applied to children which have not reached the *eighth month* of gestation. At this age, according to him, the ratio is—

Before respiration.	*After respiration.*
1 : 63	1 : 37

And in the *ninth* month:

Before respiration.	*After respiration.*
1 : 60	1 : 45

The ratio, he observes, becomes higher after respiration, in proportion to the degree of perfection with which the process has been carried on.[2]

I have founded the following table on a few cases which have come before me:

Weight of the Body.	*Lungs.*	*Ratio.*
14400 gr.	300 gr. . . .	1 : 48
33847	687 . . .	1 : 53
36000	630 . . .	1 : 57
33465	586 . . .	1 : 57
38400	640 . . .	1 : 60

[1] It appears that they were derived from observations on *two* children born dead, and *one* immature child which had breathed. Orfila, Op. cit. I. 342.

[2] Médecine Légale, I. 556. These results appear somewhat paradoxical. They lead to the inference that respiration is more perfectly set up, after a given time, in a child born at the eighth month, than in one born at the full period.

In all of these the child had been born dead, and had *not* respired during birth. The first case ought, perhaps, to be withdrawn from the list, as the child was immature. This table will give a mean average ratio of 1 : 55 for children whose lungs have not received air, or of 1 : 57 for *mature* children. The mean of three cases of children which had breathed, I found to be 1 : 35, corresponding to the ratio laid down by Ploucquet. In one still-born child, the ratio was 1 : 48; and in a child which had lived twenty-four hours, it was 1 : 41. From these remarks it will be seen that Ploucquet's test, although very far from being infallible, is capable of serving as a good corroborative proof. In the case of the child which is the subject of these remarks, the ratio was 1 : 57; and this, coupled with the other circumstances, affords evidence that the child had not breathed.

The artificial inflation of the lungs was here purposely performed, in order to complicate the case; but supposing the lungs to have been so found on inspection, a medical jurist would have had just ground for suspecting that the air had not been derived from respiration. The weight of the lungs was not altered—it was the same as in the fœtus; and the air contained within them was expelled by compression. In regard to the last-mentioned criterion, it may be asked—Is it not possible, by continued compression, to expel air from lungs that have actually respired? The answer which several experiments enable me to return to this question, is—That when the lungs have been *fully distended* with air by respiration, it is impossible to force out that air by the mechanical compression of any of the divided portions of the organs. If the pieces of lung be reduced to a perfect pulp—such as by lengthened trituration in a mortar—then, undoubtedly, the air from respiration will be forced out, and they will sink. But this is a condition which ought to be positively avoided in the performance of the experiment. If the compression be in any instance carried so far, as that the organic texture of the lung is destroyed, no safe medico-legal inference can be drawn from the result of the experiment. So far as the few observations which I have been able to make allow me to form an opinion, the air propelled into the lungs, even at the maximum degree of artificial inflation, can be expelled by compression in the manner described, without the destruction of their organic texture. The retention of air, sufficient to render portions of the organs buoyant, after very firm compression, is then, in my opinion, strong corroborative evidence of its having been derived from respiration; and thus it is advisable, whenever the hydrostatic test is employed, not to draw an inference from the floating of the lungs in water, until after the floating portions have been *firmly* compressed. The evidence thus obtained is to be regarded as only *corroborative;* because, in medical jurisprudence, it it is proper to found an opinion, not upon one, but upon all the medical circumstances of a case.

It has been said that compression will not extricate air from lungs which have *fully* respired. By this, it is not to be under-

stood that the experiment of compression can only be practically applied, to distinguish respiration, in those cases in which a child has lived for a considerable time after its birth. I have found it to succeed, even where a child had lived to make no more than one or two respirations, and had died before it was actually born. In this case, it was found necessary to destroy the child while the head was presenting, in order to effect delivery. It lived, however, a sufficient time after the protrusion of its head, with the greater part of the brain evacuated, to cry loudly for an instant. The general appearance of the body showed that it had attained to the full period of gestation. On opening the thorax, the lungs were seen projecting slightly forwards, over the sides of the pericardium. They were of a light red colour, but not crepitant under the finger. They had the external physical characters which these organs are known to acquire on the first establishment of respiration; but the entire absence of crepitation proved that the process could have been but very feebly performed. The colour of the external surface was throughout uniform—a circumstance which I have never witnessed in lungs that had been artificially inflated, except where the inflation had been carried to its fullest extent. Then, however, there is, commonly, distinct crepitation. A uniformly light-red colour, and an absence of crepitation, are, so far as my experience will allow me to speak, characters of respired, not of artificially inflated, lungs.

When removed and placed on water, these organs floated freely; and, on being separated, both appeared equally buoyant. Each lung was next divided into sixteen pieces, and every piece floated. In dividing them, it was observed that the colour was uniform throughout their substance; there was no sense of crepitus under the knife, nor could the air-cells, in which the air was diffused, be seen. The pieces were then subjected to very forcible compression, for a considerable time, in a folded cloth. The cloth was ruptured by the force employed; yet, on removing the pieces and placing them on water, they all continued to float. A portion of air had undoubtedly been forced out, but not sufficient to deprive any of them of their buoyancy.

By this we learn, that, in some instances, two or three respirations, only, will stamp upon the lungs characters by which they may easily be distinguished from those organs that have undergone artificial inflation. The compression was carried to the furthest possible limit, consistently with the preservation of the organic structure of the lungs.

It must not be supposed that, in all children which have lived but a second or two to respire, similar results will be obtained. The respiration of an instant may distend the lungs of one child, as much as respiration, continued for several hours, would those of another. The time which a child has survived its birth does not allow us to predict to what degree its lungs will be found distended on inspection, or what the results of experiments on those organs will be.

A child may have very feebly respired, and died either a few minutes or a few hours after its birth; and it is a question worthy of being examined, whether compression, sufficiently long continued, will force the air out of lungs that have become thus imperfectly distended. There is, of course, no definite boundary between the full and partial distension of these organs; but it is not strictly correct to apply inferences, derived from experiments on them in their fully distended state, to those instances in which they have really received but a small quantity of air. By this imperfect distension, I shall understand that condition of the healthy lungs in which they contain only sufficient air to render them buoyant in water; and from the slight difference in their specific gravity and that of water, a very small quantity will suffice for this. In these cases, moreover, the colour, volume, and consistency, are scarcely changed from the fœtal condition. Certain experiments, some of which will be related hereafter, have led me to the conclusion, that if lungs, thus feebly distended, be divided and compressed, they will be deprived of sufficient air to render them heavier than water; and this effect will follow without a necessary destruction of their organic texture. In most works on Medical Jurisprudence, it is laid down as a positive axiom, that air, once introduced by respiration, can never be forced out of the lungs, so long as their substance is not destroyed. It does not seem to have been imagined that the results of experiments may differ, according to the degree to which the process has been carried on. The difference I believe to depend on this: in feeble respiration, the air passes into the larger divisions of the bronchi only—not into the minute bronchiæ or air-cells. Ploucquet, Roose, Schmidtmüller, and Schmidt, long since made this remark.[1]

The admission, that air may be compressed out of feebly respired lungs by the same means as out of those which have been submitted to artificial inflation, may appear to render compression useless, as a diagnostic sign of artificial inflation; but we must not forget, that other corroborative sources of evidence may be forthcoming. The experiment of compression will distinguish all cases of complete respiration from those of artificial inflation; and, if for this circumstance alone, it ought to be regarded as an adjunct, occasionally of great value in these investigations.[2]

[1] "In solchen Fällen dringt die Luft zwar in die Luftröhre, und in die grösseren Aeste derselben, nicht aber in die kleinern Aeste und Luftzellen." Henke, Von der Lungen-und-Athemprobe. Lehrbuch der G. M. p. 375. Also Niemann. Taschenbuch der gerichtlichen Arzeneiwissenschaft, p. 91. Orfila, Médecine Légale, I. 337, 372. Meckel. Lehrbuch der G. M. 270.

[2] Orfila, who, in general, may be considered as one of the most accurate of French medical jurists, expresses himself very obscurely relatively to the effect of compression on the lungs. In one part of his excellent work (Méd. Lég., I. 358), he says:—"The air derived from respiration is contained within the bronchial cells; and can only be expelled from them (by compression) with the greatest difficulty." While at another part (p. 363) we find him asserting, "That under artificial inflation, or respiration, the air is contained in the bronchial cells; and in *neither case* can it be entirely expelled by

It must, I think, be admitted, that there are no means of distinguishing feeble respiration from artificial inflation which has been carried only to a slight extent.[1] The physical characters of the lungs will be unaltered; and compression will, in either condition, destroy their buoyancy. In a case of this kind, I apprehend the only course left open to a medical witness is, to state to the jury, that the evidence, derived from experiments on the lungs, left it uncertain whether the child in question had respired or not. The jury will then know how to return their verdict; for it must be remembered, they have always circumstances to guide their judgment, as well as medical opinions; and it is upon the whole, and not upon a part of the evidence laid before them, that their verdict is founded.

It is singular that this occasional difficulty of distinguishing artificial inflation from respiration should have been represented as a serious objection to the employment of the hydrostatic test. Even admitting, in the very few instances in which such a defence on the part of a prisoner is possible, that the practitioner is unable to distinguish the one condition from the other, this becomes purely a point for the consideration of a jury; it cannot affect the general application of the test.[2] Examples of this sort of difficulty are by no means uncommon, in the practice of medical jurisprudence. Thus, to take what appears to me a parallel case:—A man, while labouring under symptoms of cerebral disease, receives a blow on the head; he dies in five or six weeks afterwards, and a considerable abscess is found in the brain. A witness is asked to state, whether the abscess resulted from the blow, or from previous disease. He is in doubt; and he expresses that doubt to the court. But, because he is unable to give a positive opinion in this instance, he would not be justified in abandoning the ordinary rules of surgical diagnosis in any future instance of a similar kind that came before him. Again, the stomach, in a case of suspected poisoning, is found inflamed, or perforated; and a medical witness may be unable to swear, whether the appearances were due to poisoning or disease, in that particular instance. But this will not prevent him from relying, in general, upon morbid appearances, as a good source of evidence in all future questions of poisoning. Many other instances might be adduced of medical evidence becoming doubtful, from circumstances wholly independent of the

compression. Those who have contended that inflated lungs may be made to sink by pressure, can only have experimented on lungs in which the inflation had been carried to a *small extent*." He does not seem to have anticipated the answer, which might be made to his own statement; namely, that where the inflated lungs had not been made to sink, the compression had not been carried far enough.

[1] M. Devergie adopts this view, from his experiments; but he denies that the buoyancy of the lungs can be destroyed by compression. Méd. Lég., I. 571. See, also, Meckel. Lehrbuch der G. M. 368.

[2] A case is recorded by Bohn, in which the mother artificially inflated the lungs. Meckel. Lehrbuch der G. M. 368. This, so far as I know, is the only instance on record.

will of the practitioner, and over which he has no possible control. In the determination of any single point in a case of child-murder, a doubt may arise; the question relative to the respiration of the child is not exempted from this rule; but it would be the height of inconsistency to contend, that, because certain means of investigation will not always enable us to express a positive opinion, we should never have recourse to them.

I presume that, in the present day, no practitioner would trust to the floating of the lungs as a sign of respiration, before he had ascertained that the air contained in them could not be expelled by compression. The charge against an accused party is not likely, therefore, to be sustained by medical evidence of the respiration of the child, unless the child have actually respired; but it is possible that, owing to a want of evidence to characterise feeble respiration, a really guilty woman may escape. The mischief to be apprehended is not, then, as it has been often alleged, that the employment of the pulmonary tests may lead to the *condemnation* of an innocent, but rather to the *acquittal* of a guilty person. This is certainly an unfortunate circumstance; but it is one for which medical science is not yet in a condition to provide an adequate remedy.

The conclusions derivable from this case, and the observations accompanying it, appear to me to be the following:—

1. That the medical examination rendered it in the highest degree probable that this child had not breathed.
2. That there was no proof of its having been born alive.
3. That a line of *inflammatory redness* may be met with on the umbilical cord of a child born dead.
4. That the pulmonary vessels may be found *distended with blood* in a child born dead.
5. That artificial inflation is only likely to be met with, to a *partial extent*, in the lungs of new-born children.
6. That compression will expel the air from lungs inflated artificially to a small or great extent.

CASE II.

Age or degree of maturity of the child.

This child was a twin of the male sex. Its body was small, and not well developed. Its length was eighteen and a third inches; and the point of attachment of the umbilical cord was nine inches and a half from the vertex, therefore a little below the centre of its length. The hair on the scalp was abundant and long; the nails of the fingers and toes were well formed, and reached to the extremities of the phalanges. The skin was pale, except about the neck, head, and face, where it was of a diffused bright red colour. The membranæ pupillares had disappeared. The testes occupied the scrotum. The chest was narrow at the upper part, wide below, and flattened in front. The body weighed 24590 grains—*i. e.* about four pounds and a quarter.

From these data, we are perhaps entitled to infer that this child had not quite reached maturity. Its weight was much below that commonly laid down for mature male children; but this is sufficiently accounted for, by its being a twin child, and by the general want of development in its body.

Had the child lived to respire?

On laying open the chest, the thymus gland appeared unusually large, and of a pale livid colour, with a slight pinkish hue. The lungs occupied the posterior parts of the cavity, projecting forwards by their anterior margins, and covering slightly the posterior and lateral parts of the pericardium. Their colour was somewhat mottled; but the greater portion of their surface presented the usual livid hue of the fœtal organs. The right lung at the apex of the upper lobe, and side of the middle lobe, contained air in vesicles visible to the eye. This air was irregularly diffused over the surface, presenting, here and there, well-defined patches of a light red colour. The upper part of the right lung was crepitant beneath the finger; but the lower lobe was firm and solid, giving not the least sign of crepitation. The left lung seemed to contain less air than the right; but at the posterior part of the inferior lobe, in the midst of a light-coloured patch, was a *prominent vesicle* of air about the size of a grain of corn. The lungs, as well as the body, were free from all traces of putrefaction; the child not having been dead above twenty-four hours, in the month of January.

The ductus arteriosus was large, and of equal diameter throughout. The foramen ovale was as it is usually found in the fœtus; the fossa ovalis being three fourths closed by the valvular membrane. The blood, which escaped from the large vessels and cavities of the heart, was liquid and dark-coloured. Ligatures were applied to the pulmonary vessels, and the lungs were removed. Both the pulmonary arteries and veins contained blood.

The absolute weight of the lungs was found to be 562 grains, about the average of those that have not received air from respiration. The test of Ploucquet gave the following ratio:— 24590 : 562 = 44 : 1. It will be seen by this, that the result of Ploucquet's test does not lead to an inference corresponding to that derivable from the absolute weight of the lungs. The difference does not appear to me to be difficult of explanation; the body weighed much below the average of that of a mature child, for the reasons already stated; and the lungs had not participated in this want of development. The case proves what has been already remarked—that there is no necessary relation between the weight of the lungs and the body, in all children and at all periods of gestation. Had this child been of the average weight, Ploucquet's test would have given a satisfactory result; but as it was, while the weight of the lungs tended to show that the child had not breathed, the relation of their weight to the body left it doubtful.

When placed on temperate distilled water, the lungs floated, but

the right appeared more buoyant than the left. The two lungs continued to float, when separated from each other. They were now divided into a number of pieces; during the division, there was evident crepitation. On placing the pieces of lung on water, the greater number floated, while some sank to the bottom. On examining those which sank, they were not found congested or diseased; their sinking being evidently due to their not having received air. Those which floated, presented, on examination of their cut surfaces, spots of a vermilion redness in the midst of the livid or fœtal colour. The specific gravity of the organs, before division, had been ascertained to be .946. The portions which floated, were now subjected to violent compression in a folded cloth; and after this, all of them sank readily to the bottom of the vessel. Some of the pieces, however, required several successive trials, before they were rendered heavier than water.

Such, then, were the data upon which an opinion was to be raised, as to whether this child had breathed or not. In favour of respiration, we have:—1. The light red colour of the lungs partially diffused; 2. The slight prominence; 3. The partial crepitation; 4. The buoyancy in water of the larger portion of the organs. Against respiration:—1. The air-vesicles being visible to the eye;[1] 2. The absolute weight of the organs, which was not greater than the average weight of fœtal lungs; 3. the total expulsion of the air contained in them by compression. All the facts of the case are explicable, on the supposition of artificial inflation, or of feeble respiration. The child, if it had breathed at all, could have breathed but very imperfectly.

The history of this case, as it was given to me by the gentleman who attended the mother, was, that the child survived its birth about *half an hour;* and appeared, during that time, to respire. It was born in a lifeless state, and artificial inflation was immediately resorted to; but all signs of life ceased in about the time mentioned.

This statement sufficiently accounted for the mixed characters of respiration and artificial inflation which the lungs presented. But had the child not respired after birth, the characters of the lungs would, doubtless, have been similar to, if not identical with,

[1] The invisibility of the vesicles or cells containing air is mentioned, by most writers, as a proof of its having been derived from respiration. When the vesicles are large and visible, the air at the same time not being due to decomposition, the presumption is, that the lungs have been violently inflated, and that air has become thereby extravasated. This diagnostic sign is not, however, free from exceptions. I have met with a case to be presently described, wherein the air, although undoubtedly derived from respiration, was scattered over the whole surface of the lungs in large visible vesicles.

Mende, the learned professor of Griefswald, mentions this extravasation of air in the lungs, among the characters of artificial inflation. He found it in the substance of the lungs, as well as on the surface. The larger vesicles existed, according to him, at the anterior and superior parts of the organs.—Ausführl. Handb. 3ter Th. p. 391.

those found; for it is certain, that the process had not been performed for that length of time, and in that degree which are necessary to produce any well marked changes from their fœtal condition. It is also certain, that the appearance which they presented were such as are imparted to lungs by artificial inflation. This, then, is a case which shows that a medical jurist has it not in his power to determine positively that a child has respired, if the process has been only feebly performed. The air is, under these circumstances, as readily forced out by compression, as if it had been artificially introduced.

There was nothing about this child to show that it had been *born alive*—a question which is of course entirely distinct from that of its having respired; although the two are sometimes strangely confounded together, as if there were always a necessary connection between them. When the lungs are fully distended with air, and all the other characters of respiration are strongly developed in them, I am willing to allow that we have a condition highly presumptive of a child having come *entirely into the world alive*. But in all cases of feeble respiration—and these, probably, form the majority in charges of child-murder, since the child is destroyed either during birth or immediately afterwards—we have no right whatever to draw the inference that the child has been born alive, because circumstances may show that it has breathed. This feeble respiration may be always set up before birth, and the child not live to breathe after it has been entirely born. We must then, in general, look for other marks about the body, besides those of respiration, in order to say whether or not it has come into the world alive.

In this case there was no evidence of live birth. The umbilical cord presented no appearance of inflammatory redness at the point near the abdomen; it was flattened and yellow beyond; in short, it possessed no other characters than those which we might expect to find in the cord of a still-born child, after exposure to the atmosphere for the same length of time.

Had this case been wholly unknown, and the subject of a legal charge, a medical practitioner would perhaps have expressed an opinion that the child had not been born alive; but the facts prove, that such an opinion would have been incorrect, for the child survived its birth half an hour. Hence it follows, that, in some instances, a guilty woman may become improperly favoured, if we affirm, from the sinking of the lungs after compression, or from the absence of any changes in the ductus arteriosus and foramen ovale internally, or in the cut portion of the umbilical cord externally, that her child has been born dead. Unfortunately, there is, at present, no remedy for this deficiency in medical evidence. But, while waiting for satisfactory means of diagnosis, I do not see that we are entitled to express so strong an exculpatory opinion, relative to the mother, as we find generally expressed on these occasions. Facts, such as those encountered in this case, no more warrant a witness in saying that the child was born dead, than that it was

born living; but a positive opinion relative to a child's being born dead, would, in general, be given from the sinking of the lungs after compression; although there might be, for aught the witness knows, strong circumstantial evidence to prove that the child had really lived after birth, and had been criminally destroyed by its mother. The consideration of the possible occurrence of a case of this kind—of a child, such as we have here examined, becoming the object of a criminal charge—ought to make every practitioner hesitate, before he pronounces, from *negative* data, an opinion which evidence, subsequently adduced by accomplices or others, may prove to be false and unfounded. The opinion which, it appears to me, he should express, when thus situated, is, not that the child was born dead, but that there were no medical facts to show that it was actually born living; although, still, it might have come into the world alive. Children that are the objects of these investigations are often found with their throats cut, with the bones of the head beaten in, or with ligatures firmly tied round the neck; and it is, of course, for a jury to consider, why such severe and mortal injuries should be found on the person of a child, whether its lungs sink in water, after compression, or not. But if, in opposition to medical experience, the witness positively affirm, from data of this kind, that the child was dead at its birth, the jury, relying upon the correctness of this dictum, can proceed no further in the investigation.

The state of this child's lungs was such as we might expect to meet with in those children that have performed the act of respiration during birth. As this is a subject which generally gives rise to some discussion in cases of child-murder, I shall here make a few remarks on it:—1st, Respiration may be performed while the child is in the uterus, after the rupture of the membranes; the mouth of the child being at the os uteri. This is what is termed *vagitus uterinus;* its occurrence, although extremely rare, seems to me to rest upon undisputed authority.[1] 2dly, A child may breathe while its head is in the vagina, either during a presentation of the head, or of the breech. This has been termed *vagitus vaginalis.* It is not very common, but it must be set down as a possible occurrence. 3dly, A child may breathe while its head is protruding from the outlet; in this position, respiration may be as completely set up in a few moments, by its crying, as we find it in some children that have actually been born, and have survived their birth for several hours. This is the most usual form of respiration before birth. In the vagitus uterinus or vaginalis, the lungs receive but a very small quantity of air; in respiration, after protrusion of the head, the lungs may be sometimes found moderately well filled; although never, perhaps, possessing the charac-

[1] For cases and remarks, see Henke. Lehrbuch der G. M. p. 371. Zeitschrift für die Staatsarzeneikunde, 1821, Heft iii. S. 21. Also, Cyc. Pract. Med., art. Pregnancy. Orfila, Méd. Lég. I. 367. Niemann. Taschenbuch, 89. Meckel. Lehrbuch der G. M. 366.

teristic properties of those which have fully respired. The well-known occurrence of respiration, under either of these three conditions, strikingly displays the fallacy of making that process, as some have done, the certain criterion of *extra-uterine life*. A child may breathe in the uterus or vagina, or with its head at the outlet, and die before its body is born; the discovery of its having respired would not, therefore, be any sort of proof of its having enjoyed what has been termed "extra-uterine life." The death of a child which has respired in the uterus or vagina from natural causes, before its entire birth, is a possible occurrence; but its death from natural causes before birth, after it has breathed by the protrusion of its head from the outlet, is, I believe, a very improbable event. All that we can say is, it may take place; but its death, under these circumstances, would be the exception to a very general rule.[1]

The hydrostatic test is only capable of determining that respiration has taken place; it cannot show whether that process was established during birth, or afterwards. The fact of a child having the power of breathing before it is entirely born, does not therefore constitute the smallest objection to its employment; although, upon this ground, we find the use of it, in any case, denounced by many eminent men of the medical, and, I may say, by the whole of the members of the legal profession. The celebrated Dr. William Hunter remarks on this subject:—"A child will commonly breathe as soon as its mouth is born or protruded from the mother; and, in that case, may lose its life before its body be born, especially when there happens to be a considerable interval between what we may call the birth of the child's head and the protrusion of its body. And if this may happen where the best assistance is at hand, it is still more likely to happen when there is none—that is, where the woman is delivered by herself."[2] Dr. Hunter here exposes, in plain language, the fallacy of trusting to signs of respiration alone, as evidence of a child having been born alive. The truth of his remarks is, in the present day, generally admitted; and if, among medico-legal writers, we find some still treating of respiration as a proof of live birth, it is from their not having sufficiently considered the probability of a child breathing and dying before its body is entirely extruded. But, we may ask, how does the admission of these views affect a case of deliberate child-murder? A *living* child may be wilfully destroyed *before*

[1] Oberkamp, in four successive deliveries of the same female, observed that the children breathed before delivery, but died before they were born. A case of this kind also occurred to Diemerbrock. See Meckel. Lehrbuch der G. M. p. 367.

"Fieri potest ut infans, capite excluso, antequam totus excludatur respiraverit, statim vero moriatur."—*Teichmeyer Instit. Méd. Lég.* c. 24. 241. Haller remarked, that such a case was not likely to occur, even once in a thousand instances; but this, in a matter calling for legal enquiry, is no answer to the objection.

[2] On the Uncertainty of the Signs of Murder, in the case of Bastard Children, p. 33.

its body is entirely born, as well as afterwards; and if the laws of England do not contemplate the wilful destruction of a *living* child, before its entire birth, as a crime, this omission cannot be imputed as a fault to the medical jurist; nor can it at all diminish the real value of the hydrostatic test, as furnishing indisputable evidence of life. Most moralists, and perhaps legislators, might consider the crime of murder sufficiently made out, when the medical evidence showed that the child had lived, and that it was *living* when *criminally destroyed*. If, however, this do not constitute infanticide in law; and evidence be further insisted on, to set forth *where* the child was actually living when murdered—whether half protruding from the vagina, or altogether external to the body of the mother; then is the fact of a child respiring before birth, an objection rather against the principles of the law, than against the tests used to determine the presence of life. A case was tried but a very short time since, in which a child had been found with a ligature firmly tied around its neck. The medical evidence showed, I think, clearly that it had breathed; and the whole of the appearances in its body were such as to leave no medical doubt that it had died by strangulation. The judge, in charging the jury, said, if they were of opinion that the prisoner *had strangled her child before it was wholly born*, she must be acquitted of the murder. The prisoner was acquitted.[1] However we may regard the question of the utility of the pulmonary tests, we cannot but look upon that law—to speak in the mildest terms—as but very imperfectly adapted to its purposes, which makes the proof of *murder* to rest, not upon the actual and wilful destruction of a *living* child, but upon the precise moment which a murderer may select for the accomplishment of the crime. Impunity is thus held out to all offenders who destroy living children in the act of birth; but there is an additional evil, accompanying the operation of this legal rule, which seriously affects medical evidence, given on these occasions. The law will presume, until the contrary appear from other circumstances, that the respiration of a child, if proved by the best of evidence, was carried on before it was entirely born. Let the witness, then, in a case of alleged infanticide, ever so clearly establish the fact of respiration, and therefore of life, at the time the violence was used, this evidence is not sufficient. He is asked whether he will depose that the child had

[1] This case proves—if any proof of the fact were wanting—with how little severity the verdicts of juries press upon individuals charged with this crime in England. From a case related by Poilroux, it would appear to be much the same in France. A trial for child-murder took place before a court of assize in that country, in which it appeared that the child was healthy, and had undoubtedly breathed. Its mouth and fauces were found stopped up with a plug formed of oak leaves and dung, from which cause there was every reason to suppose it had died. The mother was found guilty of having caused its death by *negligence*. "After such a verdict as this," observes the reporter, "we might be almost justified in burning all that has been written on the subject of infanticide."—*Poilroux, Traité de Médecine Légale Criminelle*, p. 155.

breathed after its body was entirely in the world. Unless he can make this deposition—which, for obvious reasons, he cannot often be in a condition to do—the law will presume, that, although the child had breathed, it came into the world dead. In this way, we perceive, a shield is effectually thrown around those who are really guilty of having destroyed their children immediately after birth; but, under any moral consideration of the circumstances, I think it impossible to admit, that a woman who kills her child in the act of birth is less guilty of murder than she who chooses the moment of its entire expulsion to destroy it.

A German medical jurist, Dr. Brefeld,[1] has lately proposed to vary the gradations of crime in child-murder, according to the degree to which respiration has taken place. He divides the beings, which are the objects of the charge, into—1, The unborn *fœtus* (*frucht*), which has not breathed. 2, That which is born, and has breathed, but so imperfectly as to render the fact incapable of demonstration; this is what he terms *a fœtal child* (*frucht-kind*). And 3, That which is born, and has fully respired—*a child* (*kind*). How long soever a child may have lived after birth, he proposes to punish its wilful destruction according to the degree to which its lungs may have become distended with air. It does not seem to me that legislators would be justified in following such an hypothetical division as this. Whether a child has breathed perfectly or imperfectly, its wilful destruction ought to be regarded as murder. The proposer of this plan assigns no reason *why* it should be regarded as a smaller offence to destroy a child whose lungs are, owing to accidental causes, only half filled with air, than one in which the organs have become fully distended.

The conclusions which we may derive from this case are:—

1. The characters presented by the lungs of this child, might have depended upon artificial inflation, upon feeble respiration, or upon both states combined.

2. That there are no satisfactory means of distinguishing artificial inflation from feeble respiration.

3. That there was no satisfactory evidence that this child had breathed; and therefore it follows, that a child may live and breathe for *half an hour* after birth, without its being discovered by a medical examination.

4. That life, prolonged for half an hour, may make no difference in the state of the ductus arteriosus, or foramen ovale.

5. That there was no evidence to show whether this child had been born living or dead.

CASE III.

This was a male child, which was examined about thirty-six hours after death.

[1] Beitrag zur Lehre vom Kindermorde, besonders in Beziehung, auf die Revision der Königl. Preuss. Strafgesetze. Henke. Zeitschrift für die S. A. 4. 1836.

Age, or degree of maturity.

Externally, the whole of the body appeared plump and well developed. Its length was about nineteen inches and a half. The umbilical cord was inserted a little below the centre. The hair was full, of a brown colour, and at least an inch in length. The nails of the fingers and toes were completely formed, and reached to the extremities of those parts. The skin was smooth, and of a pale white colour; but over the face, chest, and sides, there were large well-defined patches of cadaverous ecchymosis, bearing the closest resemblance to those discolorations which are met with in the bodies of persons hanged or suffocated. The palms of the hands and soles of the feet were deeply livid. On the abdomen, the skin was slightly green, from incipient putrefaction; but the epidermis was in every part firmly attached to the cutis. The umbilical cord was tied, and dressed.[1] Between the ligature and the future point of separation at the abdomen, the cord was flattened, horny, and transparent. The umbilical vein, contracted to a dark thread, was seen traversing the transparent part. The membranæ pupillares had disappeared. The testicles were in the scrotum. The body weighed 35930 grains, about six pounds.

From these facts, it was inferred that the child was either at, or very near, the full period of gestation.

Had it lived to respire?

The thorax was remarkably full, arched, and expanded. On opening this cavity, the lungs were seen of a dark livid colour, slightly mottled in places; but in no situation were they of a light-red hue, nor did they present any where the peculiar colour imparted to these organs by the process of respiration. They projected but very little forwards, covering, and partly concealing, the posterior and lateral parts of the pericardium. On pressure between the fingers, there was a dull and indistinct crepitation; which became more marked, when a portion of the blood, with which they were evidently congested, had been forced out. On examining the lungs more closely, it was seen that, scattered over the surfaces of both, especially anteriorly and externally, there were numerous vesicles of air, varying in size, but most of them not larger than a millet-seed. The air within them was apparently only retained by the pleural covering. The vesicles burst, and the air escaped, on very slight pressure.

The heart was large, and the pericardium contained an unusual quantity of serum. The right side of the organ was much dis-

[1] This, although it can be scarcely ranked among medical circumstances, is strongly corroborative of a child having been born alive. Why, indeed, it may be asked, should the umbilical cord ever be found tied and dressed in a child, which was actually *still-born*? This may be set down as part of the presumptive evidence in a case; but a medical witness must endeavour to conduct his enquiries as if he had no presumptive evidence to guide him.

tended with venous blood. The pulmonary vessels contained a quantity of dark grumous blood. The ductus arteriosus was also full, and of the same diameter throughout. The foramen ovale was placed nearly in the vertical line of the fossa: it was freely open: there was not the smallest appearance of contraction in the aperture. In fact, this and the ductus arteriosus were in their usual fœtal condition. The thymus gland was large, and of a deep livid colour.

The lungs, when removed, weighed 774 grains; and by Ploucquet's test the following ratio was obtained:—35930 : 774 = 46.5 : 1.

They were next placed on distilled water; and they slowly sank to the bottom of the vessel, where they continued, after moderate compression; although by this, a quantity of blood was forced out, and the water became deeply coloured. It was observed that a number of minute air-bubbles escaped from the trachea, and rose to the surface on the moment of their immersion. Two or three incisions were made into them, from which blood flowed freely; and they were allowed to remain in water, in a cool apartment, for twenty hours. At the end of this time, it was found that they had risen to the surface, and were floating, although slightly below the level of the water. In the mean time, it was evident that a quantity of blood had issued from them, for the water was highly coloured. They were now pale, but more crepitant than at first.

The lungs were then separated; and it was observed that each floated. They were divided into a number of pieces. There was crepitation on cutting them; and air obviously escaped from the cut surfaces, by compression under water; but little blood now followed the incisions into their substance. Some of the divided portions, when placed in water, floated; others sank; while others, again, seemed to be in equilibrium. Those pieces which floated, when slightly compressed between the fingers without the use of a folded cloth, rapidly sank. In this way, every portion of the organs was ultimately rendered heavier than water. On examining the pieces which sank in the first instance, there was no appearance whatever of disease. The structure of the lungs was, throughout, perfectly healthy, so that their sinking could have been alone due to their not having received air. On dividing them, they were found of a uniformly venous red colour. Before making any remarks upon the data, which we have thus obtained, to solve the question of respiration, I think it right, as there is much that is peculiar in this case, to state, that the child had been born alive—that it had *lived six hours* after birth, the greater part of which time it was in strong convulsions. It seemed to suffer from oppression at the chest; and made the most violent efforts to respire. Since it appeared active and strong, no attempt was made to inflate the lungs artificially.

Let us first enquire, whether it could have been inferred, from the experiments on the lungs, that this child had respired? When first examined, they sank in water, notwithstanding that they were *crepitant*, and *obviously contained air*. Perhaps, if they had been

at this time divided into pieces, some would have been found to float, and others to sink; although the organs were much congested with blood—a condition that might have counteracted the effect of the air. But the subsequent experiments satisfactorily show, that, even had any portions then floated, they would have been easily made to sink by compression; since this, applied only in a slight degree afterwards, caused every piece to fall to the bottom of the vessel. A circumstance worthy of remark is, that the lungs, although exposed in water during the night in a cool apartment, should have risen to the surface, and floated twenty hours afterwards. One of two causes must have operated to produce this change: 1. Gaseous putrefaction. 2. The slow removal of the blood, congested in them, by the water: in which case, they would easily acquire buoyancy, from the air that they contained.

It appears to me, that gaseous putrefaction cannot be admitted as having been the cause, for the following reasons. The lungs were perfectly fresh and sound, when placed at the bottom of the water, within the short period of twenty hours before. Although the weather was warm, the vessel was kept in a cool apartment; and access of air was cut off from the organs, by the water in which they were lying. Lastly, when removed, and cut to pieces, there was not, on any portion of them, the slightest smell or discoloration analogous to putrefaction. In truth, if gaseous putrefaction were admitted to have been the cause of this after-buoyancy, I do not see how we are to distinguish those organs which are putrefied from those which are not. That the second cause was the more probable, may, I think, be hence inferred. The lungs were, at first, but little heavier than water: they sank very slowly, so that their specific gravity obviously differed, in a very slight degree, from that of water. From the great discoloration of the water, although the organs had been well washed previously to immersion, it was evident, that a quantity of blood had been abstracted from them; certainly, it might be imagined, sufficient to make all the difference observed in their specific gravity. From this view it follows, that sanguineous congestion was most probably the cause of their sinking in the first instance; and although compression was employed to force out the blood, it was not carried sufficiently far to render the organs buoyant. It is to be remembered, that extreme compression, in lungs which have feebly respired, will have the disadvantage of forcing out all the air, as well as the blood contained in them; so that this means of distinguishing the sinking arising from congestion may, unless employed with caution, occasionally defeat the object of the investigation. The better plan, probably, is to make a few incisions into their substance, and allow them to remain in water for two or three hours, without resorting to compression. In this case, no more pressure was employed at first, than was sufficient to expel the blood from the larger pulmonary trunks.

In answer to the question proposed, it will be observed, that we have no satisfactory proofs of this child having breathed. The physical characters of the lungs were not those indicative of respi-

ration: neither the colour, nor the volume of the organs, afforded any evidence of the fact. Their absolute weight was but little above the average of those of the fœtus. The test of Ploucquet left the question entirely doubtful. The ductus arteriosus and foramen ovale had not experienced the least alteration. The only circumstance in favour of respiration was, the feeling of a dull crepitation in the lungs, on pressure; but the air was diffused, in *visible vesicles*, over the surface of both organs; a condition which, if I may judge from my own limited experience, is rather an unusual mode of distribution for air received by respiration. Had I not known the facts of the case, I should have considered this appearance strongly indicative of the lungs having been artificially inflated; more especially, since all the other physical characters of respiration were wanting. But these lungs had certainly not been artificially inflated; and, therefore, the manner in which air is found distributed in these organs is not to be always regarded as a sure indication of its origin.

One striking fact was, their sinking when first experimented on, although this child had lived and breathed for six hours after its birth. A reason for this has already been assigned; but still, it is a point worthy of fixing the serious attention of medical jurists. It shows most clearly that buoyancy of the lungs is not a necessary consequence of a child having lived and breathed for some time after birth. Probably, had this been a case calling for medico-legal enquiry, the lungs would have been cut to pieces and experimented on at once: this, at least, is the usual course. The sinking of the divided pieces, either before or after compression, would have been set down as negativing the act of respiration, and, unless other strong evidence were forthcoming, the fact of the child having survived its birth. Here, again, we perceive the necessity of not assuming that a child has been born dead, because its lungs sink in water. There may be no good medical evidence of such a child having lived after birth; but assuredly the mere sinking does not warrant the common dictum, that the child was necessarily dead, when born[1]: it would be as reasonable to pronounce, in a question of poisoning, that the fact of an individual having died from poison was negatived by the non-discovery of a poisonous substance in the stomach of the deceased. How would such an assertion, as this, be justifiable, on the part of a medical jurist? Yet a line of conduct, which would be denounced in the one instance, is almost constantly acted upon, in the other[2].

Medical evidence, in a case like this, would have wholly failed to establish the fact of respiration; for the only circumstance in favour of the performance of the process, namely, the presence of air in the lungs, was rather weakened by the manner in which that

[1] There are several cases on record, in which, although the lungs sank in water, the mothers confessed that they had destroyed the children after birth. Meckel. Lehrbuch der G. M. 365. For cases, *vide* Briand Méd. Lég. 251. Annales d'Hygiène, 1837. I. 407.

[2] See Orfila, I. 417.

air was distributed. In short, the appearances were those of artificial inflation; and would probably have been pronounced to have resulted from this cause, by most examiners; but still there was nothing to refute the possibility of respiration having been feebly performed. The state of the ductus arteriosus and foramen ovale, upon the contraction of which, as evidence of life after birth, so much stress has been laid by some writers, was the same as in the fœtus. The change in the diameter of the duct has been described as an immediate consequence of respiration; and it has been even laid down, that where we find its diameter unaltered, on an inspection of the body, we may pronounce that the child has not respired. The case preceding this, shows the fallacy of trusting to such exclusive views; but the present affords, perhaps, a still more striking instance of the error into which a medical jurist may fall, by adopting such hasty generalisations.

Considering the question of respiration altogether distinct from that of a child having been born alive, it will be necessary to determine, whether, in this case, we had any good evidence of live birth. It has been already remarked, that the signs of full and perfect respiration in the lungs are presumptive of live birth; but these signs were, here, altogether wanting; therefore the condition of the lungs afforded no evidence of the fact. The child might, or might not, have survived its birth for a certain period. In the same way, the ductus arteriosus and foramen ovale were not perceptibly altered, so that no evidence could be derived from the state of these parts. Marks of violence on the body very often lead to a presumption, which a witness should not neglect. Thus, supposing wounds evidently vital, and probably simultaneously inflicted, be found on different and remote parts of a child's body, this is presumptive of the whole of its body having been alive in the world, at the time they were received[1]. In the case under examination, there were no wounds or bodily injuries; there was merely diffused lividity, such as is usually witnessed in death by suffocation; but this was not of itself sufficient to allow of a safe opinion being formed. Exfoliation or desquamation of the cuticle is mentioned among the external signs of survivorship: this appearance was here wanting. Lastly, certain changes in the umbilical cord are set down as furnishing evidence, not merely of live birth, but of the time within certain limits, that a child may have survived. Thus it is said, that in a child which has not been born alive, or has died immediately after birth, the umbilical cord will be found firm, rounded, thick, of a bluish colour, and more or less spongy to the feel. In about twenty-four hours after birth, supposing the child to live, it becomes shriveled and contracted, from the point at which the ligature is applied, to where the cord joins the abdominal parietes. From the second to the third day after birth, the cord becomes brown in colour, and dry between the ligature and abdomen: it also acquires a certain degree of semi-transparency: its

[1] See Meckel. Lehrbuch der G. M. 354.

three vessels are seen flattened, contracted, and containing only a slight filament of coagulated blood. From the third to the fourth day, the colour deepens, and the cord becomes semi-transparent, and more flattened: the umbilical arteries are almost obliterated: the vein is contracted, but pervious. From the fourth to the sixth day, the cord usually separates from the abdomen. Such is the account, given by M. Devergie, of those changes which, it is presumed, will enable a medical jurist to solve the difficult questions connected with live birth and survivorship[1].

In the case before us, the portion of cord between the ligature and abdomen was flattened, horny, and transparent; the umbilical vein contracted and dried up, as it were, to a filament of coagulated blood. This is precisely the condition which is laid down, as evidence of a survivorship of three days. It is true, that the examination of the body was not made until thirty-six hours after death; but the cord had been well wrapped up, so as not to be exposed to the free access of air; and I do not apprehend, therefore, that any considerable change had taken place in its physical characters, from the time of death, until the body was examined. We learn by this, that the changes in the umbilical cord follow each other in a manner too uncertain to allow of any opinion being expressed relative to the time which a child has actually survived its birth. Here the average changes of *three days* were met with in a child which had not lived more than *six hours*. But the point to be examined, is, whether the changes in the cord of this child were such as to allow us to express an opinion that it had been born alive, and had lived after birth. It appears to me that they were. Supposing the child to have been dead when born, the cord would not, I think, have been in the condition in which it was found. It might have become more dry; or, as the weather was warm, it would rather, perhaps, from its being fully impregnated with liquid matter, have passed into a state of putrefaction. It is not probable that in forty-two hours from birth it would have been found dry, hard, and transparent, or that the umbilical vein would have become contracted to a mere filament of hardened coagulated blood, unless the child had lived for a certain time after birth. Experience has, it appears to me, well established, that the changes in the cord of a dead child, within a given period, are, *cæteris paribus*, very different from those which take place in that of a living child. I have, on several occasions, observed a striking difference in the cords of children, exposed for the same period, at the same time, under similar circumstances, when the one was still-born, and the other had survived its birth some hours. The observation, then, of this part of the body may occasionally allow a practitioner to draw a presumption of a child having been born alive, even when the organs of respiration and circulation afford not the smallest evidence of the fact.

[1] Médecine Légale, I. 513.—M. Devergie seems quite sensible of the numerous exceptions to which these rules for determining survivorship are liable.

This case appears to me to lead to the following conclusions, important in a medico-legal view:

1. That the lungs of a child, which has lived and breathed *six hours*, may sink in water, and present none of the physical characters of respired lungs.
2. That air, from respiration, may be diffused in visible vesicles over the surface, and throughout the substance of the organs.
3. That air, from respiration, may, by very moderate pressure, be forced out from divided portions of the organs.
4. That a survivorship of six hours after birth may make no difference in the fœtal condition of the ductus arteriosus and foramen ovale.
5. That the divided portion of umbilical cord may undergo those changes in six hours, which are often not witnessed until after the lapse of two or three days.
6. That these changes sometimes afford evidence of live birth, when all other evidence fails.

CASE IV.

In this case the child was a twin of the female sex.

Age, or degree of maturity.

Its body was seventeen and a half inches long. The umbilical cord was attached to the abdomen, about nine and a half inches from the vertex; its point of insertion was, therefore, about an inch below the centre. The hair was tolerably abundant, and long. The nails were not perfectly developed—they did not quite reach to the extremities of the fingers. The skin was universally pale, except that of the face, which was slightly suffused. There was not the least sign of putrefaction about the body. The skin of the umbilical cord presented a red inflammatory mark, about a quarter of an inch from the abdomen; beyond this, and as far as the situation of the ligature, about an inch, it was yellow, desiccated, and semi-transparent. The body weighed 27686 grains—not quite five pounds.

These circumstances led to the inference that the child had not quite reached maturity; due allowance having been made for its being a twin child.

Had it lived to respire?

The chest was small, contracted at the upper part, and flattened anteriorly. Upon laying open the cavity, the lungs were seen somewhat full, and slightly projecting forwards, but not sufficient to cover the pericardium in any part. They were of a deep livid colour, like those of a still-born child; their surface presented, in some parts, minute granular spots of a lighter hue, but still bearing no resemblance to the colour derived from respiration. On firm compression, they were not found to be in the least crepitating, in any portion of their structure.

The heart was unusually large, and the pericardium contained a quantity of serum. The ductus arteriosus was large, of equal diameter throughout, and evidently not reduced from its fœtal dimensions. The aperture of the foramen ovale was situated in the upper and right part of the fossa; the thin membranous partition closing, as usual, about three fourths of the space; its crescentic margin having the axis of its concavity directed upwards and to the right. Ligatures were applied to the pulmonary vessels; the arteries were small, but they contained blood.

The lungs, when placed on distilled water, with the heart attached, rapidly sank to the bottom of the vessel, the heart turning uppermost. No portion of the lungs seemed buoyant. The lungs, separated from the heart, were found to weigh 675 grains; and the ratio, obtained from Ploucquet's test, was, therefore, 27686 : 675= 41 : 1.

The specific gravity of the organs was taken, and found to be 1.046; thus differing, in only a fractional degree, from that of the liver or lungs of the fœtus which has not breathed.[1]

The lungs were now separately placed on water, but they both sank with equal rapidity. Each lung was then cut into fifteen pieces. The substance of the organs was healthy, of a deep Modena-red colour, with here and there patches of a somewhat lighter hue. There was no crepitation under the knife, nor was there any mark of congestion; for no more blood followed the incisions than is ordinarily witnessed in dividing the lungs of a fœtus. The pieces of the two lungs, having been kept apart, were placed on water separately; and it was remarked that every portion sank rapidly to the bottom. The lighter-coloured masses were now cut out from the divided pieces, and placed on water; but there was no difference in their specific gravity—they all equally gained the bottom; and, on compression below the surface of water, no bubbles of air escaped.

Probably the answer returned by a medical witness from these data would have been, that the child in question had not breathed, but that it had been born dead. The fact, however, was, that this child had not only been born alive, but had survived its birth *twenty-four hours*. No particular remark was made respecting its respiration.

It is true, that the condition of the heart and lungs could not have led to the remotest suspicion of the child having respired—much less of its having lived for so long a period after birth; but we learn from this, what the previous case had already taught us, that active life is not always necessarily indicated by obvious physical changes in the lungs; in other words, that there are instances in which the ordinary resources of medical science wholly fail to elicit the fact. Unable to explain how it was that the life of a child

[1] According to Albrecht Meckel, who has performed many experiments on this subject, lungs that have not respired are one fourteenth heavier than their bulk of water. (Lehrbuch der G. M. p. 354). This would give a specific gravity of about 1.071.

could be continued after birth without its lungs becoming permanently distended by the process of respiration, the older medical jurists were accustomed to deny the correctness of reports of this kind: they thought that there was no better means of defending the employment of the hydrostatic test, than that of meeting all alleged obstacles to its use by a positive denial of their existence. It is needless to say that this spirit of opposition has long since disappeared: enquirers into these subjects have now found that the best way to support a doctrine is, to try it by every method of investigation—to show its defects as well as its excellences, and to submit it to the most rigorous analysis. Experience has also taught them, that to invest any doctrine with a value which does not belong to it, only gives it a temporary importance, and subsequently exposes it to a total downfall. It often happens, indeed, when fallacies are once shown to exist in a doctrine, that individuals pass from one extreme of opinion to the other—from credulity to absolute scepticism. Thus, even the cases in which the pulmonary tests might be useful are now lost sight of, in the number in which they have been improperly employed.

A few observations have already been made upon life, as manifested in children under the form of imperfect respiration, in which the lungs, in some part of their substance, retain sufficient air to float, provided they be not congested or diseased. The question is more intricate, however, when we find that the *healthy* lungs of an infant sink, although it may have lived and breathed some time after its birth. The sinking, in such a case as the present, could not be ascribed to sanguineous congestion or any diseased condition, but to the absolute want of air. In the first place, it is to be observed, the lungs may contain air, and yet be heavier than their bulk of water; since, although the specific gravity of the unrespired organs differs but little from that of water, it requires a certain quantity of air within them to give them buoyancy. The mere fact of sinking, then, is no evidence of their not containing air; for this sinking will often be observed when they are placed in water entire, while portions will be found to float when they are divided. The case here examined was so far remarkable, that not a single portion, although the two lungs were divided into *thirty pieces*, floated; even the subdivided portions of some of these sank. It is clear, therefore, that not one *thirtieth* part of the lungs of this child had received air; indeed, neither by compression under water, nor in any other way, could I detect the presence of air in any part of them. These cases are ordinarily set down as the exceptions to a very general rule, and perhaps they ought to be regarded in this light; but, at the same time, I cannot help thinking that they are more common than some medical jurists are inclined to admit. In examining the body of a child, the history of which is unknown, it is proper that the possible occurrence of such cases should be well borne in mind. It appears to me not improbable, that many such come yearly before coroners in this country; and that they are dismissed as cases of still-born children, notwithstanding the

severe marks of violence which are often found about the bodies. If, as I have already observed, the lungs sink in water, the fact is regarded as sufficient evidence of still-birth. This is assuredly putting the most humane interpretation on the circumstances; and so far the result is not to be objected to: but we should take care, in carrying out this principle, that we do not thus throw obstacles in the way of judicial enquiry.[1]

The time which this child survived was twenty-four hours. Professor Bernt met with an instance, in which a seven months' child died two hours after birth; and when its lungs were divided and placed on water, every fragment sank.[2] Remer has reported another, in which the lungs sank in water, both entire, as well as when divided; although the child had survived its birth at least four days.[3] In this case, the navel string separated naturally before death. Orfila found, in a child which had lived eleven hours, every portion of the lungs, when divided, to sink on immersion. In three other cases, in which the subjects survived birth, four, six, and ten hours, the lungs also sank when divided; two of these were mature children.[4] Other instances are recorded by Daniel, Schenk, and Osiander. Metzger[5] supposed that premature children alone were likely to present this anomaly—*i. e.* of continuing to live after birth without leaving any clear signs of respiration in their lungs. Perhaps the greater number of these cases have occurred among premature children; but the observations of Schenk,[6] Remer[7], and Orfila, satisfactorily prove, that perfectly mature children may also be the subjects of this singular condition. It is of course presumed that there is no cause of disease to render the lungs heavier than water; but that their structure resembles that of the lungs of children which have not breathed.

[1] If we take *twenty* cases, in which the deaths of new-born children require the investigation of a coroner, it will, I think, be found that at least *fifteen* of these children are pronounced, by the verdict given either at the inquest or trial, to have been *born dead*. This is equivalent to seventy-five out of one hundred. Now, what are the real facts? Statistical tables, extending over a series of years, and embracing not less than eight millions of births, show that the still-born do not form above one eighteenth or one twentieth of the total births; *i. e.*, not more than from five to six out of one hundred. As we might suppose, however, in *illegitimate* births, among which the majority of cases of child-murder unquestionably lie, the average number of still-born is augmented; but, taken over a period of twenty years, in the city of Geneva, these did not constitute more than twelve per cent. of the births. Males are more frequently born dead than females, probably from the greater size of the head and body. In Prussia, the still-born males were to the females, during a period of fifteen years, in the ratio of 1.35 to 1; in Geneva, during a period of twenty years, as 1.33 to 1.—*British and Foreign Medical Review*, *July*, 1837.

[2] Cummin on Infanticide, p. 65.

[3] Henke. Lehrbuch der G. M. 374.

[4] Méd. Lég., I. 375.

[5] System der gerichtlichen Arzeneiwissenschaft, 403.

[6] In Schenk's case, the child weighed six and three quarter pounds, and was nineteen inches in length. It lived four days.—Niemann. Taschenbuch der G. A. p. 93.

[7] Bernt. Systematisches Handbuch der gerichtlichen Arzneikunde, p. 243.

The occasional existence of this state of the lungs of the living child is then placed beyond all dispute; the explanation of the causes upon which it depends—how it is that a child may live and breathe for hours or days, and that no sign of respiration be discovered in its body after death—is involved in great difficulty. The late researches of Dr. Joerg, of Leipzig, have, however, thrown some light upon the subject; and these may probably lead the way to other discoveries in this obscure department of physiology. Some of Dr. Joerg's views are peculiar. He considers that the act of parturition, as well as the *duration* of the process, has a material influence upon the system of a child; and that they serve to prepare it for the efforts which it has to make in performing respiration. I quote from the analysis of his paper by Dr. Graves.[1] "A parturition of the natural duration gradually checks the placental circulation, and limits that of the fœtus chiefly to its own system; while it engenders in the latter a gradually increasing, and, finally, an urgent, want of some new mode of respiration. If the act of parturition be much shorter in duration than is natural, the child incurs the danger of being born in other respects healthy, but not at the moment endowed with the organic stimulus to expand its chest for the purpose of making the first inspiration." Supposing the first inspirations to be, from any cause, feeble or imperfect, then the organs will become only partially distended; the remaining portions will preserve their fœtal condition. Dr. Joerg considers this as a positively diseased state of the lungs in the new-born child, and he has given to it the name of "*atelectasis*."[2] It may proceed from various causes. He considers that children which are born after a very easy and rapid delivery are liable to it; and thus it may be found in a mature, as well as in an immature child. Any cause which much weakens the vital powers of a child before its actual birth, may give rise to the occurrence of this imperfect dilatation of the lungs. In this way, it may be due to long continued pressure on the head during delivery, or to hemorrhage from the cord. All the causes of asphyxia in a new-born child will, when operating only in a very slight degree, also produce this atelectasic condition. When only a part of the lungs becomes, in the first instance, distended, the child may not afterwards acquire sufficient strength to fill the remaining portions; it may thus live on some hours or days, respiring at intervals, and becoming occasionally convulsed, in which state it will probably sink exhausted, and die. The doctor has remarked, that those portions of the lung which are not speedily distended by air, afterwards become consolidated or hepatised, so that all traces of their vesicular structure are lost. The length of time which the child survives, will depend upon the degree to which its lungs have become dilated.

It is not necessary that the whole of the lungs should have received air, in order that a child should continue to live even for

[1] "On the Fœtal Lungs in New-Born Children." See Dublin Journal of Medical Science for July, 1836. Also, Medical Gazette, Vol. XVIII., p. 604.

[2] ατελης, "incomplete;" εκτασις, "expansion."

some months after its birth. A few years ago I met with the following case, which will serve to illustrate this statement. A child, aged *six months*, had, it was supposed, been destroyed by suffocation. Upon opening the thorax, the viscera were found healthy; but the whole of the inferior lobe of the right lung was, so far as regarded colour, density, and structure, precisely like the lungs of the fœtus; no air having ever penetrated into it. It had become developed in size, but its vesicular structure was perfectly destroyed. When the whole of the lung was placed in water, it floated; but when the inferior lobe was separated, it immediately sank to the bottom of the vessel. I have no doubt that this was a case of *atelectasis*, such as it is described by Joerg. The lobe had not received air in the first instance; and had become afterwards consolidated or hepatised, so that it could not be inflated.

To apply these remarks to the case under examination, I think we must admit, that this child's lungs were wholly in a state of atelectasis; into the probable cause of which it is not here my purpose to enquire. These researches of Joerg show that a medical jurist, besides attending to the post-mortem appearance in a child submitted to examination, should also, if possible, ascertain whether the delivery has been rapid or not. A knowledge of the facts connected with delivery may sometimes enable him to offer an explanation of this singular condition of the lungs, when it exists in a child which is otherwise healthy and mature.

When the hydrostatic test fails to indicate respiration, it is easy to understand, that all the other signs of an altered circulation will be absent. However long the child may live, the absolute weight of the lungs, as well as the relation of this to the weight of the body, will not vary materially from that of the fœtus. Further, the ductus arteriosus and foramen ovale will retain their fœtal characters; indeed, it is not improbable that the blood continues to circulate though these parts after birth, since the great cause of the diversion of the current of blood, the process of respiration, has not come into full operation. Hence, then, there was not a single medical fact which could have led to the presumption that this child had respired.

We shall now see whether there were any better grounds for presuming that it had been born alive. The state of the umbilical cord was the only circumstance which could be looked to as likely to assist in this part of the enquiry. The skin had a line of inflammatory redness at the point of future separation; and between this and the ligature it was yellow, desiccated, and transparent. But little stress, it appears to me, can be placed upon this *line of redness*, as evidence of life after birth, since it was met with in Case 1, wherein the child came into the world dead. To the desiccation and transparency, I think more importance may be attached, for reasons already assigned under Case 3. If we are acquainted with the circumstances under which the child's body has been exposed, we may, from the discovery of these conditions, draw a presumption, that the child has lived after birth; although, it must be con-

fessed, this would be scarcely admissible in law, unless it were corroborated by other less equivocal evidence. That these slight changes are capable of indicating the period of survivorship, is not to be admitted.

On the whole, this may be set down as another instance, in which, if the child had been criminally destroyed, medical evidence would have entirely failed in showing that it had lived; although the sinking of the lungs, as I have already observed, would not have entitled the witness to say that the child must have come into the world *dead*. A criminal would have had the benefit of the unavoidable deficiencies which yet exist in this department of science.

The conclusions to which this case leads, are:—

1. That a child may live and breathe for *twenty-four* hours, and yet every part of its lungs, although healthy, may sink in water.

2. That the lungs of a child which has lived twenty-four hours may present all the characters of those organs, as they exist in a child which has come into the world dead.

3. That life, protracted for twenty-four hours, does not necessarily alter the condition of the ductus arteriosus and foramen ovale.

4. That there are instances in which medical evidence will not establish that a child has breathed, or survived its birth.

It will be seen, on an examination of the four cases here related, that three of them are well calculated to show how inadequate the pulmonary tests are, in some instances, to establish those points which many consider them capable of proving. Of these three cases, one renders it certain, that a degree of respiration, sufficient to give buoyancy to the lungs in water, cannot be demonstrated upon clear and satisfactory grounds;—there being no means of distinguishing it from air artificially introduced. The two last, on the other hand, show that life may be prolonged for many hours, without the lungs becoming buoyant from respiration. It may be remarked of these tests, but more especially of that founded on the floating of the lungs in water, that medical practitioners have differed much, at different times, in their ideas of what it was fitted to prove. About fifty years ago, it would seem that the hydrostatic test was regarded, by some, as capable of furnishing evidence of murder. Thus, we find Dr. Hunter asking the question, "How far may we conclude that the child was born alive, *and probably murdered by its mother*, if the lungs swim in water?"[1] Later

[1] Op. cit. p. 28.—The titles of many of the works published before Dr. Hunter's time bear evidence of the mistaken views of the authors as to what the hydrostatic test was capable of proving. In 1722, Heister published a treatise, called "Programma quo ostenditur ex pulmonis fœtûs innatatione vel submersione in aquâ, nullum certum *infanticidii* signum desumi posse." See Orfila, I. 418. Niemann. Taschenbuch, 87.

authorities, and, indeed many in the present day, assert that it is capable of proving whether a child has been *born alive* or not. From what has already been stated in this paper, as well as from the most simple reflection on the circumstances accompanying the birth of children, I think it must be evident, that the hydrostatic test is no more capable of showing that a child has *been born alive or dead*, than it is of proving whether it has been murdered, or has died from natural causes. The majority of those who have made experiments on this subject have only pretended to show, by the use of this and the other tests, whether or not *a child has breathed;* and a very slight examination will render it apparent, that in no case are they susceptible of doing more. But even here, their utility is much restricted by numerous contravening circumstances, a knowledge of which is essential to him who wishes to make a practical application of the facts connected with them. If we were asked to state in what cases the pulmonary tests are capable of assisting the medical jurist, I think the answer would be :— 1st, They will clearly show that a new-born child has lived, when, during its life, it has *fully and perfectly respired.* Cases of this description form a certain number of those which come before our courts of law. To them, the most serious objections are not applicable: and the few which might be made to the medical inferences are not difficult to answer. 2dly, They will allow a witness to say that the lungs must have either received air by respiration, or by artificial inflation. These are the cases in which a child has died soon after birth, and where the respiratory changes are but very imperfectly manifested in the lungs. I believe that they form the large majority of those that fall under the jurisdiction of the criminal law. It might be considered that the qualification, in the inference here made, neutralised its force ; but it must be remembered, that there are few instances of *actual and deliberate child-murder*, wherein artificial inflation could become even a *possible defence* for an accused party. So unusual is this kind of defence, that, among the numerous trials for infanticide which have taken place in this country for many years past, I have not been able to meet with a single instance in which it was alleged, in answer to the buoyancy of the lungs, that the prisoner had inflated them in order to resuscitate her child. The reason is obvious: had such a defence been attempted, the whole of the circumstantial evidence would at once have set it aside.[1] When, in the suspected murder of an adult, a medical man swears that a fatal wound was such as that the deceased might have inflicted it on himself, or that the prisoner might have produced it, he is placing the jury in a very similar position to that in which he places them in a case of child-murder, when he says that the child might have breathed, or its lungs might have been artificially inflated. How would a jury decide in the two cases? Assuredly, by connecting together certain facts with which a medical witness has no concern, but which

[1] See Niemann. Taschenbuch der G. A. 89.

may, in their opinion, satisfactorily supply the place of what is defective in his evidence. It is not for him to calculate the probabilities of respiration, or of artificial inflation; but it is for them to consider, whether an accused party was likely to have resorted to an experiment of this nature. The hydrostatic test ought not, therefore, to be lightly condemned or rejected upon an abstract objection, which, in nine tenths of the cases of child murder, could not possibly exist. Let it be granted to the fullest extent that a conscientious medical jurist cannot draw a positive distinction between respiration and artificial inflation—still, the jury may be in a situation to relieve him from the difficulty. In short, it would be as reasonable to contend that all murderers should be acquitted, because *homicidal* are not always to be distinguished from *suicidal* wounds, as to argue that all cases of infanticide should be abandoned because these two conditions are not to be known from each other by any certain medical signs. If juries do frequently dismiss such cases, it is, I apprehend, to be ascribed rather to their great unwillingness to become the means of administering very severe laws, than to their want of power to balance and decide on the probabilities laid before them. The second case related in this paper may be taken as a fair example of the doubtful condition in which the lungs will be found in numerous investigations respecting infanticide.

Cases 1, 3, and 4, represent a peculiar class. They are beyond the reach of the pulmonary tests; because the lungs do not receive and retain a perceptible quantity of air, although, as in 3 and 4, the subjects may have lived some hours. The hydrostatic test is no more capable of showing that such subjects as these have lived, than it is of indicating from what cause they have died. The facts have been already sufficiently commented on. Cases of this kind, although not so common as the preceding, demonstrate that existence may be for some time continued under a state of the respiratory process, not to be discovered after death. In the opinion of many, these cases form a serious objection to the tests; but it is difficult to understand how they can affect the application of them, in the instances alluded to; or why, because signs of respiration do not always exist in the lungs of children which have lived, we are not to rely upon them when they are actually found. Poison is not always discernible after death in the stomach of a person who has taken it; but this does not prevent a medical jurist from searching for it, and relying upon its discovery under proper cautions, as evidence of poisoning in any other case.

These singular instances prove that we are greatly in want of some sign to indicate life after birth, *when the marks of respiration are absent*. Until we discover this, we must of course make the best use of that knowledge which lies at our disposal; taking care to apply it to those cases alone to which experience shows it to be adapted. In the mean time, the inference that a child has been born dead because its lungs sink in water, although not physiologically correct, is never likely to implicate an innocent party: it

may sometimes cause the liberation of the guilty. But to recommend the abandonment of the pulmonary tests on this account, would be equal to proposing that the whole code of criminal law should be repealed, because it does not always succeed in convicting and punishing those who have infringed its provisions.

If the pulmonary test were wholly set aside, it is easy to conceive what would be the consequences. Thus, let us suppose that a new-born child is found, under suspicious circumstances, with its throat cut;—we are called upon to say that it is impossible for medical evidence to establish whether the child had lived or not, and therefore we are to decline making an inspection of its body. But this would be the same as declaring that child-murder could never be proved against an accused party, and that new-born children might henceforth be destroyed with impunity. It appears to me, that conduct of this kind, on the part of a medical witness, would be wholly unwarrantable; for we may sometimes acquire, by an inspection, as great a certainty of respiration having been performed, and therefore of a child having lived, as of any other fact of a medico-legal nature. Cases of poisoning often give rise to greater difficulties to a medical jurist; as where, for example, he attempts to found his opinion of the cause of death on symptoms or post-mortem appearances. But we will put the question in this light. In the body of a healthy full-grown child, which has but recently died, we find the lungs filling out the cavity of the chest, of a light red colour, spongy, and crepitant beneath the finger, weighing at least two ounces, and, when divided into numerous pieces, each piece floating on water, even after violent compression. Is it possible in such a case to doubt that respiration had been performed? If there be no certainty here, it appears to me that medical experience is but little fitted to guide us in our enquiries. It would be difficult to point out an instance in which an affirmative medical opinion would be more surely warranted by the data upon which it was founded.

Again, it has been contended, that the difficulties accompanying the tests are of too delicate a nature for a practitioner to surmount; that they are only serviceable in the hands of those who have had frequent opportunities of experimenting. Even admitting this reasoning to be true, the inference rather forms an objection against those who employ the tests, than against the tests themselves. On the same principle, the analysis of poisons would have been long since abandoned to a few. A very short experience has however shown the injustice of this view, in regard to toxicology; and I feel certain that a few years will prove it to be equally unfounded in relation to infanticide.[1]

[1] For the best and most impartial summaries of the merits of the pulmonary tests, I must refer to Meckel. Lehrbuch der gerichtlichen Medicin. Halle, 1821. Also to the Cyc. Pract. Med.; Art. *Infanticide.*

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